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### MIND AND BODY, FROM THE GENETIC POINT OF VIEW.<sup>1</sup>

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In the discussion of the relation between body and mind, we often find the position advanced that the experience of each is, in its original form, equally 'subjective.' Idealistic analysis of external objects reduces them to sensational and other presentational elements. It thus becomes possible to take what Professor Sanford, in his president's address printed in the last issue of this REVIEW,<sup>2</sup> calls the 'purely psychological'<sup>3</sup> point of view, in the consideration of this time-honored problem.

The direct result of such a point of view is that we have to deal with what may be called psychic or subjective series alone, and the remote consequences, for the theory of knowledge, are the familiar ones common to the different forms of subjective idealism.

Apart altogether from the results which follow for the discussion of the relation of body and mind as indicated in detail below, I wish to discuss briefly the legitimacy of this purely subjective point of view; not so much with reference to the traditional question of the validity or sort of reality attaching to the external world, as with reference to the supposed primacy of the subjective. Does it follow that because the

<sup>1</sup> Lecture read before the Yale Philosophical Club on February 10, 1903, and also before the Princeton Psychological and Philosophical Seminars.

<sup>2</sup> THE PSYCHOLOGICAL REVIEW, March, 1903.

<sup>3</sup> A better designation would be 'purely psychic'; that is, the point of view of the subject of the experience.

world as well as the self, the body as well as the mind, is a construction from data of presentation—that therefore, the subjective factors are entitled, in some sort, to greater primacy and ultimateness with reference to the universe as a whole than those which we ordinarily denominate objective and external?<sup>1</sup> I propose to look at the question from the point of view of the genesis of the distinction between body and mind; and in the light of our outcome to consider the general question of the relation of the two sorts of reality to each other.

## I.

There has been considerable discussion lately as to the rise of this main distinction. As to the outcome, the chief results are, I think, very generally accepted—apart from differences of detail. In the first place, we may say that consciousness, in its earliest experiences, does not have the distinction between the 'inner' and the 'outer,' the self and the world. Its experience is what I shall call in a figure 'protoplasmic'; it is in Ward's phrase 'a continuum,' or in James' phrase 'a buzzing confusion'—the two writers agreeing, in their differences, that classification and differentiation of the sort requisite for the separation of the worlds of the subjective and the external, are absent.<sup>2</sup> Experience of this neutral or protoplasmic sort, when considered as preliminary to the later distinction, I have called, using a term which goes well with 'subjective' and 'objective,' by the term 'projective'—the positive side of the conception being this, that even then a mental content is presented or 'projected' in the field of the respective sense through which it arises.

The beginning of the distinction between mind and body seems to be found—assuming that experience is at first undifferentiated or protoplasmic—in the child's apprehension of its presentations as being respectively of or from *persons*, on the

<sup>1</sup> I use 'external' as opposed to subjective, since the term 'objective,' applying as well to mental objects which are not external, would be ambiguous.

<sup>2</sup> See the joint art. 'Experience,' in the writer's *Dict. of Philos.*, in which James goes further than either Stout or the present writer in maintaining the virgin neutrality of experience of the earliest type, with reference to 'inner' and 'outer.'

one hand, and again *things* on the other hand. If we use the symbol 'Pr.' for projects, or presentations *not yet distinguished* as external things or as self, we may put it down that the first distinction is thereafter between 'P,' persons, and 'T,' presentations which are not persons — understanding, however, that in the child's apprehension this distinction is merely a distinction, not a characterization, and that it comes to him largely through the accommodations of his active life to the varying situations in which he finds himself.

He next, through a process various aspects of which are described by various authors under the terms 'introjection,' 'imitative interpretation,' etc., assimilates his *P's* — his personal projects — to certain other intimate, pungent, highly toned experiences. These are set over against other presentations separated off as '*T's*,' or thing-projects. The *P*-experiences have a series of aspects which I can not now describe in detail,<sup>1</sup> but which comprise effort, and pleasure and pain, and belong in a mysterious way to the individual himself. Things, he learns, may be left behind, may be shared, may be manipulated: and again things resist, limit activity, stand stubborn — characters, all of them, which the *P's* do not so evidently show. Here, in this growing distinction, is the germ of the difference between that which may be *left behind and found again* — the external — on the one hand; and the ever present, the always owned — the subjective — on the other hand.

Now what it is essential to note at this stage of the progress of consciousness is this: the *P*-experiences, which by this act of interpretation have become 'subjective,' are at once and by the same act of apprehension, *common to the individual's psychic self* — which I shall call '*S*' — *and to the other self or projective person already presented in contrast to things*. The self-as-subject arises by interpretation of the objective *P*-presentation. This carries with it a further interesting implication.

The child has heretofore apprehended his own body as a thing — a projective mass of sense experiences, like other things. He now distinguishes the *P*-characters from the *T*-char-

<sup>1</sup> It may be said that it is not necessary for our present purpose to describe the actual materials which go into the notion of personality; of that there may be great diversity of opinion.

acters in certain things. Further, he claims the *P*-characters as being his also — as being identical with certain of his own states realized as subjective. So he treats his own body precisely as he treats other objects, and finds his subjective self-part, *S*, distinguished from his mere thing-part, *T*. The thing-part in all persons, in others and also in himself, becomes differentiated from the *S*-part. This thing-part he comes later on to call 'Body,' while the self-part he calls 'Mind.' The *S* is one of a number all of which are Mind, *M*; and the things which go with selves, being equally numerous, are Body, *B*. Here then we have discovered the great line of cleavage or segregation, as between mind and body, in its essential genesis.

The implications at this point are more or less evident. Just as at the earlier stage, in reaching the distinction between the subjective and the external, we found that the genesis of the self essentially implicates other selves, and cannot be apprehended without them, so now, with respect to the relation between mind and body. What I have called the thing-part of the person is realized, as in relation to the self-part, first in the case of other persons; but the relation is necessarily carried over to the private self, who is also considered as mind associated with body. Expressing these implications also in symbols, we may say that *M* (my mind) is thought only with *M'* (other minds), and *B* (other bodies) only with *B'* (my body), and that when one entire personality is set over against another, *M* is associated with the *B'*, and *M'* with *B*.

If this general line of genetic distinctions<sup>1</sup> be accepted, and we then ask what consequences would seem to follow for the discussion of the relation between mind and body, we may, I think, draw conclusions of some importance. Before proceeding, however, with the discussion it may be well to make precise the symbolism we have been hitherto employing, and to set down the stages of mental growth recognized above, in order to show

<sup>1</sup> In support of such views the reader may consult Royce, 'Studies in Good and Evil' (the chapters on the Self); Stout, 'Manual of Psychology,' Bk. IV., Chap. VII.; Mezes, 'Ethics, Descriptive and Explanatory,' Chap. VII.; Ormond, 'Foundations of Knowledge,' Pt. II., Chaps. VII., X.; and the writer's 'Social and Ethical Interpretations,' Chap. I., and 'Mental Development in the Child and the Race,' Chap. XI.



the sort of opposition or contrast which obtains at each stage between the two terms to the controversy, mind and body. This is done in the symbolic expressions immediately following.

The symbol  $()$  is herein suggested to denote the 'becoming' or genesis of the term which follows it, from that which precedes it: it may be called *the symbol of genesis*. Its interpretation in particular cases would be alternatively in one in three modes: genesis which is indefinite as to exhaustiveness in the analysis of  $A$  into  $B$ , for which the expression is of two equal semicircles, thus  $A()B$ ; second, genesis expressed as  $A$  exhausted in  $B$ , but not necessarily  $B$  in  $A$ , in which case the semicircle toward  $A$  is greater than that toward  $B$ , thus  $A()B$ , third, genesis expressed as  $A$  not exhausted in  $B$ , in which case the semicircle toward  $A$  is less than that toward  $B$ , thus  $A()B$ . We here have occasion to use only the first of these, the symbol of 'indefinite genesis, or genetic progression.'<sup>1</sup>

Summing up the stages of growth, now pointed out, in symbolic statements, we have certain expressions which I shall call 'Progressions,' in distinction from 'Equations.' The sign for genesis may be rendered by the word 'becomes.'

Progression 1.  $Pr$  (projective experience)  $() P + T$ .

Progression 2.  $P() S + S'$ .

Progression 3.  $\frac{S'}{T}() \frac{M}{B'} + \frac{M'}{B}$ .

We may characterize these 'Progressions,' respectively, each in view of its place in the entire genetic process, as (1) the 'projective Progression,' which reads, *Projects become Personal-Pr and Thing-Pr*; (2) the 'subjective Progression,' which reads, *Personal-Pr become Subject-self and Object-self*; and (3), 'the ejective Progression,' which reads, *Object-self becomes Mind and Body*—the last alone representing complete dualism of Body and Mind.

## II.

So far we have come to a dualistic stage in the development of consciousness, and we have intimated the conclusion that from this stage of interpretation, the two terms,  $M$  and  $B$ , must

<sup>1</sup> I may emphasize the distinction intended between the values of these signs and those of signs of quantity.

be considered as strictly correlative. The determination, in the course of reflection, of further predicates for either the one or the other, if made at all, should be of such a sort that some further relation between them takes the place of this. This first and most general result requires that we must take either *the original or the derived form of experience* — the earlier stage or the later stage of conscious growth — as the basis of examination and criticism. If we take the early or protoplasmic stage, we lack the distinction altogether; and we have then to say that the question is an impossible one, since the terms of the antithesis between mind and body have purely abstract or logical value. If, on the other hand, we take the later stage, when the distinction has matured, then the two terms, mind and body, *have strictly correlative meanings*, and in recognizing the sort of predicates which consciousness and reflection attach to the one, we are bound, by right of genesis and meaning, to do the same for the other also.

Suppose that, in our further quest, the point of view described above as the 'purely subjective,' be taken; we have then to ask the question as to what form the concepts mind and body take on in the thought of the individual thinker whose point of view it is. We find that to think of body as presentation is, in accordance with Progression (3), to think other minds with it as presentation, and this involves, by Progression (2), thinking of one's own mind as presentation. In other words, it is impossible on this hypothesis to take any other than a purely phenomenalist or presentational view of both sorts of object, body and mind. The procedure which involves treating other minds as objective phenomena, and at the same time maintaining the psychic point of view with reference to one's own mind, is illegitimate.

Furthermore, to think of another mind as presentation, and to maintain also the psychic point of view, is to think of the other mind as thinking of the thinker as presentation. So there results, at the best, the notion of a series of isolated psychic centers. On this basis no general thought or theory of mind and body could be derived; for to do so would be to desert the strictly private standpoint, and bring back that of phenomenal correlation of body and mind.

If then we intentionally adopt the point of view of correlation we find two alternatives: either we have to consider the relation of the presenting mind to its object, the presented body (including by implication also the body which is associated with the presenting mind) — the relation of *M* to *B* (or to *B'*) — or, on the other hand, we have to consider the relation of presented mind to presented body — that of *M'* to *B*.

The first of these problems would seem to be the traditional question of the relation of mind to its object: the question of theory of knowledge or epistemology. In so far as it is merely the construction of a mental object which is in question — the sort of function in exercise in the perception of the external object — we have no approach one way or the other to the question of mind and body; for whatever the result, not only does body become experience, but mind, as presenting, is also in some sort experience. And as soon as we reach the apprehension of the presenting self as some form of experience we come to the other alternative — as is symbolized in Progression (3) — that of the relation of mind as presented to body as presented. It may be said that there is a mode of self-experience of which the self is not presented as content. That view represents a later stage of reflection, I think, and it is taken up below (in the next paragraph but one).

The other form of this alternative, however, that of the relation of a mind to its own body, is an interesting case here, for we assume the brain in the body, and by our Progression (3), this *B'* is an implication of *B* (the body or brain of another which is actually presented). This is often thought to import into the question some peculiar features, for it is supposed that our knowledge of our own brain is in some way less direct, and hence, in some way, less valid and reliable than that of other brains which we might actually see if they were put before us. I am quite unable, however, to see any force in this position; for the brain in connection with another person's mind is not actually seen, but is only inferred as part of the whole of his organism of which we see the other parts; and exactly the same is true of the thinker's own brain. He does see and touch his own organism in certain features the same as those which he

experiences of other persons' bodies, and besides he has from his own body those direct sense-impressions upon the basis of which his original distinction between the subjective and the external is actually based. We may accordingly treat the two cases as really one, and accept the result stated immediately above, to the effect that the case is reduced to that of *the relation of presented body to presented mind*. We have still, indeed, two cases, one of which is that in which the presented mind is actually a different mind and the other that in which the presented mind is the same as the presenting mind, and it may be well before going further to inquire into the meaning of this distinction. It is important to do this because, in the result, we shall find that confusion on this point lends some unclearness to the analysis usually made from the subjective standpoint.

When we refer, as is done above, to the relation of the presenting to presented mind, for the purpose of the discussion of parallelism or of any other general theory, what we mean by presenting-mind-as-itself-presented is the individual mind made the object of its own presentation. We do not mean the mind as simply conscious operation upon the body presented. This latter is very often meant; as, for example, when we are told to take the point of view of the mind's spontaneous perception of body, and ask in what sense the presenting mind is then and there conscious of itself. We must take the meaning to be the individual mind as made the object of its own presentation, that is as an object additional and alternative to body likewise presented as object; because the theory requires that the view reached should cover the case of the relation of another person's mind to his body, and that would mean his mind presented as object to an onlooker in the same sense that his body is presented as object. Psychologically, indeed, the difference is that between unreflective and reflective consciousness. When it is argued that there is a form of apprehension of self as function, not as presentation, and that this apprehension is felt or in some way realized in the act of presenting body as object, we are, as I take it, abstracting from the act of reflection and in so far reducing consciousness to the stage — in the matter of the distinction of body from mind — of our Progression (1), at

which the distinction between subjective and external is not yet realized, or is so obscure that the self is not presented as distinct from other subjective selves. This latter stage, that of Progression (2), allows the distinction of self and other-self only because it permits the full and explicit presentation of the experiences common to selves as such.

If this be true, namely, that to take the subjective point of view, in the sense of supposing a conscious self-function over against body-content—that this really requires or involves the spontaneous point of view, then we must allow ourselves only the distinction between mind and body which consciousness has achieved at that stage of its development. Now we find that consciousness has then only projective distinctions, not subject-object distinctions; and if that be the stage for valid interpretation, then the question of the sort of reality attaching to either of them over against the other cannot be put at all.

I wish to insist on this result. It is fundamentally that of the general criticism made on the subjective standpoint at the end of the first or genetic section of this paper above, except that we now reach it by detailed analysis. It brings the direct charge against the subjectivists of confusing the spontaneous with the reflective points of view. Only a genetic analysis of these stages—as shown in the Progressions (1) and (2)—can expose this confusion. If we treat the body-member of the antithesis as being such a product of reflective thought, that its reality, its properties, etc., can be shown to be due to the higher activities of distinction and synthesis which characterize the reflective consciousness, then we must treat the mind, in which this thought of body occurs, as exercising these reflective processes in apprehending body. But if we do this, we are dealing not with mind as spontaneous function but as having itself as object or content of reflection set over against such a body; in other words we must take Progression (3) as the basis of interpretation. If, on the other hand, we treat mind as being the individual's spontaneous function of construction of an object, then the object, body, must be the sort of object, the 'project,' which such a consciousness is able to construct. The fallacy of the subjectivists is in saying that in contrasting body and mind,

we may mean a thought of body which is a constructed object subject to analysis, and a thought of mind which is not an object at all. Of course, if such a procedure be allowed, we may at once say : body is a complex of mental states, mind is the agent which has the states — and the conclusions of idealism follow.

But suppose we turn the tables, and commit ourselves for a moment to the diametrically opposite procedure. Suppose we say : mind is the inferred thing which, upon reflection and as the result of objective analysis, we suppose to be the bearer of psychic states ; body, on the contrary, is the immediately given, the original mode of sense-experiences of resistance, etc. : hence the primary of body. Mind turns out to be a series of epiphenomena. This is the fallacy of the materialists. Its roots are found just in taking the spontaneous standpoint for one term of the antithesis, body, and the reflective standpoint for the other, mind.

I contend that both procedures — that of the subjectivists no less than that of the externalists — that both are illegitimate. If we interpret mind and body by Progression (1), the antithesis between them does not exist, and we have no data for such a question ; if we interpret both by Progression (3), in which the dualism has emerged, we have the question of the relation between presented mind, considered as the object of reflection, and presented body, likewise considered as an object of reflection. This alone is the legitimate form of the scientific and philosophical problem.

### III.

This question is that of the relation of the two general spheres of objective construction to each other. This question, as I conceive, demands further genetic analysis, for it involves the determination of the sort of objective reference attaching to each of the two modes of construction, together with their content ; also the categories in which each content is organized. In short, if we are to ask as to the reality of mind and body, and as to the sort of real relation which subsists between them, our only possible solution resides in the determination of the nature of the concepts of reality or objective validity attaching in our reflection to each.

This, of course, is a 'large order' ; and we cannot hope to



develop a theory of reality here. Yet we may assume acquaintance with the alternative historical points of view, and ask what value the genetic approach to the concept of such a real relation gives to either term over against the other. Particularly may we expect light, from the genetic progress of consciousness, upon that possible interrelation of the two spheres of reality which is involved in such a theory as that of 'interaction.' In anticipation, I may say at this point that the considerations which follow are directly opposed to the theory of interaction, and in favor of a form of psychophysical parallelism, finding its philosophical ground in a reflective monism.

When we come to ask as to the genetic procedure in the organization by which the two spheres of reality, respectively designated the mental and the physical, are constituted, we find that the processes called above 'Progressions' are to be recognized. The fundamental ground of the distinction between persons and things resides in the need of classification and reaction upon contents which have such different characters that they cannot be treated without such a fundamental differentiation. I think it is a fair statement of the generally accepted views of the two sorts of science arising from this differentiation to say that physical science is a form of knowledge in which the materials are those which can be organized under the category of 'agentic,' or what are usually called 'mechanical,' sequence. These materials are distinguished very early in the mental history of the child from the genetic or amechanical sequences, by their character as showing a certain *regularity*, *invariability*, and *reversibility*, which beget in him an attitude of discounting or predicting their occurrence and recurrence. This is the sphere of repetition, and in the reactive life, the sphere of habit. This mass of material is the domain of those retrospective, exhaustively formulated sequences which admit of measurement, and which, in their further pursuit, yield results in quantitative physical science. I do not mean to say that all physical science, all knowledge of the external world, is actually quantitative; but what I mean to say is that this aspect of the world of external objects constitutes it a mode or sphere of reality different from the mental. It is the ideal of physical science to be age-

netic and mechanical;<sup>1</sup> this is the category which it exploits. In so far as this world has aspects which cannot be so treated, we find an actual tendency to divorce such aspects from the external and to read them into the subjective group—as, for example, sense-pleasures and pains, so-called subjective sensations generally, and in a less degree, odors and tastes—certain of which, under the various designations ‘secondary properties,’ etc., are thought to be more loosely attached to external objects and more nearly subjective than other properties. Yet they are apprehended by direct stimulation from external objects, in the same way that other physical properties are.<sup>2</sup>

Such an agenetic form of sequence is what is meant by ‘cause and effect’ in the physical sciences. As soon as we attempt to make that notion mean more than temporal sequence, by introducing into it the idea of efficiency, we reach the concept of energy, which is quantitatively determined, and liable to the agenetic formula of ‘conservation of energy.’ This has led to a monopoly of the concept of cause by the physical sciences and its formulation under such dicta as ‘there can be nothing in the effect that is not already in the cause’ and the construction of the phrase ‘post hoc ergo propter hoc’ in the sense ‘non propter hoc non effectus.’ In other terms, we find the physical formula for cause and effect to be strictly an *Equation*, not a *Progression*, in the genetic meaning of Progression as indefinite inequivalence. The assertion of such an inequality is considered *ipso facto* the denial of causation.

Furthermore, this equality view of causation is dominant even in the attempts at genetic deduction of the category of cause. The experience of effort is looked upon as intensively graded in proportion to the difficulty of the movement accomplished, or to the resistance overcome in the external world.

<sup>1</sup> This means, finally, reducible to motion; and since motion is always reversible (whether the actual reversal is within our power or not) we have *reversibility* as the criterion of a strictly agenetic or mechanical series.

<sup>2</sup> It is interesting to note the development of this sort of construction in the history of the scientific impulse, *i. e.*, a lingering personification of the forces of nature wherever they are mystifying or not subject to regular behavior. The history of science shows the gradual reduction of the personal to the mechanical in external phenomena; but to-day spiritism remains the religion of mystified physicists!

And the next and final step in the 'physicallizing,' so to speak, of the conception, and with it of the experience on which it is based, is that the mind is supposed to be in causal interaction with the body in a way which is expressible in an equation: so much mental expenditure or experienced force equal to so much physical effect or work — muscular work, and, remotely, purely mechanical work.

It suffices, at this point, to make this fact plain — the appropriation of the notion of cause by physical science, and the very general tendency of philosophers to accept it. I shall come again to the point further down in our discussion; here we may for the present rest it, and return to the consideration of the corresponding genetic development of the notion of mind and the categories under which in turn it is constituted.

The beginning of the distinction between body and mind — as shown in our Progression (I) — is the contrast, in presented phenomena, between the regular and predictable, and the irregular and, in a sense, capricious. It is facts of the latter sort that the child finds imitatively available, and in general assimilable to those aspects of his experience embodying his own agency and his private subjective interests. These go on to be organized in the form of self, over against the antithetic organization of the phenomena of body, as sketched in what immediately precedes. For our present purpose — which is not an exhaustive statement, but merely the development of a contrast — the essential character of this type of his experience is its genetic organization. It is in the experiences of impulse, instinct, strain, effort, etc., that his personal self comes up sharply against the regularities and law-abiding changes of the physical world; and his mental growth is a series of achievements by which he reduces the conflicts, and effects a *status quo* for the practice of the essential things of his life. He grows by accommodations, personal concessions, adjustments, which become systematized in a progress of mind more and more complex, and to the onlooker, essentially novel. His instincts — the most mechanical part of him — are broken up; his impulses are inhibited; out of all the conflicts of processes that wonderful endowment of personality, voluntary self-control and determination, emerges

and becomes an instrument of foresight and prudent conduct. All this is progress—that is the point; not mere repetition. It is growth, not mere change. It is genesis, not a reversible atomic series. In type, therefore—and this is what the term means—it is genetic, not mechanical, not agenetic.

Mind, therefore, when he generalizes it, is something which is characterized through and through by growth, genetic organization. As he looks upon another person, saying of him, 'body and mind,' by mind besides body, he means a subjective part which is, like his own subjective part, a thing of growth in accommodation and self-determination. And when he says of a person that he has both body and mind, he means what he means also of himself—a being which somehow has two sides, each of them showing a characteristic type of serial change, illustrated in many particular cases. And these two sorts of change, whenever realized in a particular case, *are uniformly and continuously together*.

Now it is in this realm—that of subjective change and determination—that the mental and moral sciences find their data. The stage of reflection shown in the Progression (3) is that of dualism. The *M*-terms are instances of one mode of sequence in phenomena, and the *B*-terms are instances of another. But it is a singular and compelling fact that the *M*-terms are, in their very nature, not capable of formulation in what the writer has elsewhere called 'cross-sections'—that is by the mere analysis of a complex situation into its elements, considered as giving an exhaustive statement in terms of a foregoing or after-following situation. Such cross-sections are possible only of physical sequences. But if they be impossible mental processes, then only 'longitudinal sections' are possible: the actual statement of Progressions, as that term is defined and illustrated above.

It results that the mental sciences must always be unfinished. Their data cannot be formulated in universal statements. Only actual growth can reveal, in each case, the next succeeding mode of organization. Such concepts as we do work out, therefore, as applicable to mental change, are essentially of this 'longitudinal' character—that is they are Progressions. Cause

and effect in psychology, for example, can only mean a sequence of indefinite inequivalence between a preceding and a following term—that is, *so far as it is genetic in type*. When we say—or when the self in the case says—that the mind causes the movement of the arm, what is meant is that the experience of the moved arm is the genetic issue of the mental processes in certain directed volitions. We do not mean to say, or we should not, that cause in the physical sense, defined above as motion followed by motion in a reversible series, is all that takes place. We mean the full statement of fact of a connection between changes in the two series, in the form of a regular sequence. If we go on to interpret the sequence by either of the two conceptions of cause, we naturally fall into one or other of the two fallacious interpretations described above.

It is interesting to note that the naïve form of interpretation of the relation subsisting between mind and body is that of genetic or longitudinal sequence, not that of quantitative transverse equivalence, although the reverse is often assumed to be the case. This fact exhibits the obverse side of the movement, spoken of above, by which the notion of cause in the physical sense gradually extends itself to include mental change. We find, in the progress of racial thought, that earliest of all, the changes of nature generally are attributed to personal agencies; that this is gradually limited as natural knowledge advances; that the segregation of phenomena, under the concepts of law and necessity, narrows the sphere attributed to personal agency; until—and this is the last stand of the naïve point of view—it is only in the one case of the relation of *one mind to one body, and that its own*, that such a point of view is still held. In the theory of interaction, the attempt is made to justify this one remaining case.

But those who make the attempt do not see that this is to preserve, in the one case, the point of view which in the progress of racial reflection has been given up in other cases. Such a position is in opposition to the essential progress of physical science. In all other cases knowledge of the physical demands, for the constitution of a lawful universe, that sporadic interferences from the sphere of mind shall be abolished. Such inter-



ferences remain as the extreme resort of religious mysticism, by which, indeed, they are called 'miracles.' In such cases, they are, even to religious thought, the exception which proves the rule. They are miracles—phenomena fit to excite wonder—just because they constitute the departure from the operation of the processes natural to the group of phenomena in which they show themselves. But in the case of the relation of body and mind there still remains, in the conception of causal interaction, the confusion of the unreflective and reflective points of view due to the immaturity of the distinction between the two spheres of causation necessary to the consistent development of either sort of science.

The evolution of contemporary thought is making this evident. This appears in the greater difficulty one has in accepting the theory of interaction when the purely objective point of view is taken—that is, when one considers the case, not of the subject's own body, but of that of another. When one takes such an objective standpoint, it is found necessary to preserve at once the integrity of the physical series of brain changes, fulfilling the principle of conservation of energy, and at the same time to allow that it is in some way interfered with by mental agency. The result takes on—apart from the resort of subjectivism—one of two forms: either the express denial that the mind increases or diminishes the amount of physical energy already in play; or, on the other hand, the resort to devices by which the modifications in the quantitative determinations of physical science, due to mental activity, may be compensated for. On the one hand, we have the claim that the mind can direct, or switch off, or hold in the physical energies; or that it can decide as between alternative expressions of physical energy, without altering its quantity—suppositions long since and many times shown to be absurd. As type of the latter sort of devices we have the suggestion attributed to Lotze that the plus and minus increments to physical energy, due to mental interference, will in the long run counterbalance each other, and the actual equilibrium of the physical forces remain after all undisturbed. All such superficial hypotheses seem to me only to keep up the confusion which it is my present purpose to expose.



I may now sum up the results of the foregoing discussion.

*En résumé*, therefore, we find that in its actual genesis, the distinction between phenomena of mind and body, considered as distinct types of presented phenomenal change, requires the use of two distinct categories of construction, the genetic and the agenetic. Physical science it is which interprets the agenetic. Its explaining concept of cause is illustrated only and always in transformations of energy. On the other hand, the mental and moral sciences interpret the genetic, in the special realm denominated subjective.<sup>1</sup> They recognize, just by their distinction from the physical, the type of change which issues in progressive organization, and permits only of longitudinal or 'progressive' statement. They contrast the *Progression* with the *Equation*. The attempt is often made to interpret by one of these categories, that of physical energy, the relation between mind and body; this leads to that form of the theory of 'interaction' which accepts the energy view of causation. Mind becomes a form of energy. This, properly speaking, is a materialistic conception. On the other hand, the attempt is made to interpret the relation between mind and body as a case of Progression or genetic change; we then perforce deny that the antecedent brain state in a psychophysical sequence fully determines the subsequent brain state, and thus allow that this case is the one miracle which survives in physical nature. This is the other form of the interaction theory. Each of these attempts makes appeal to a stage of relatively naïve or unreflective consciousness. Both fall into the same confusion; but they illustrate the confusion differently. One maintains the point of view of reflection only on the side of the physical; the other only on the side of the subjective.

We have maintained, on the contrary, that the theory of the relation of mind and body must maintain consistently the opposition under which alone these two concepts develop and have validity; it must be free, in explaining the relation between the two, from the application to it of either category which belongs exclusively to one of them.

<sup>1</sup> The sciences of life lie between. Life processes are really genetic, as is maintained below.

## IV.

With so much game in our drag-net, we now have to ask the final question as to the proper apparatus for fishing in the deep seas of the mind's more refined reflection. The problem is this: Can we hold each set of phenomena to its own legitimate construction and at the same time reach a comprehensive conception\* of the concomitance of mind and body under which the scientific formulas appropriate to each may be given full value?

So far as the formulation reached at the stage of scientific dualism represented by Progression (3) is concerned — the point of view common to psychology and physics alike — it is expressed in psychophysical parallelism; for it is the essence of that theory that it refuses to postulate any positive predicates of the psychophysical relation, and rests content with the recognition of sufficient uniformity and generality to justify investigation by recognized scientific methods in both departments of science.

More positively, indeed, psychophysical parallelism justifies itself genetically at both the stages of mental development shown respectively in Progression (2) and Progression (3). We may take the point of view of Progression (2), and deliberately refuse to allow the validity of the dualism reached by reflection in Progression (3). In that case, the phenomena of personality are simply joint phenomena; neither mind nor body is treated under peculiar categories. The interpretation of a sequence is from one psychophysical term — what we may call a *BM* term — to another; and we have no possible question of the separate action of brain-states or mind-states as determining subsequent states of brain or mind or both. We then have the simple form of Progression, *BM()* *B'M'*, in which one psychophysical term, *taken as a whole*, is considered as the antecedent of another, also *taken as a whole*.

This is to my mind, both in biology and in psychology, the only justifiable scientific method. Its merit is that it compels the equal recognition of the two aspects of phenomena whenever they are both present, and really banishes the futile question of reconciling the terms of a dualism which, so far as concrete phenomena are concerned, is abstract and artificial. I have

argued in detail for this conception in my recent work, 'Development and Evolution,' claiming that general biology suffers equally with genetic psychology from the divorce of the facts in these two allied spheres, which the traditional dualism compels. Evolution is psychophysical, not organic and *besides* — or possibly, *not at all* — mental. The psychophysical standpoint is the only valid scientific standpoint for a theory of organic descent, no less than for a theory of individual development; and the two genetic series must be interrelated by some form of 'inter-genetic concurrence.'<sup>1</sup> The question, of course, remains over as to the scientific formulations possible to such Progressions; that is as to a method of investigation of successive 'genetic modes' of organization.

If, however, we commit ourselves to a point of view which, as in Progression (3), results in an explicit dualism, then our care must be to reach interpretations which do not invalidate, though they may transcend, this dualism. I have shown above that the current forms of the interaction theory involve a confusion of categories, due to the failure to maintain a consistent level of mental development. The only outlet is to push reflection further, and find a category of experience within which the two forms of sequence may proceed, indeed, as demanded by reflective dualism, while still, at the same time, held in a single thought without conflict. Our question then becomes: How can we satisfy the mental demand for a type of change which shall, at one and the same time, both exhibit the form of 'progression' by successive genetic organizations and also be liable to interpretation in terms of the equations of agenetic science? This to my mind — as I have said in formulating it differently in a recent discussion — is *the* question of contemporary philosophy. I can of course, do no more here than indicate certain seemingly valid approaches to its solution.

In the first place, empirical science must do its utmost in actually finding and examining such complex sequences. But its first qualifications are to be its competency and its fairness in seeing

<sup>1</sup> An expression used in the work cited for the relation existing between racial evolution and individual development, of which the law of 'recapitulation' is the broadest biological formulation.

them when they are found. As a matter of fact they do not lie far out of hand. It was an insight of Aristotle that the phenomena of life contain the great things of philosophy. Life shows both the *dunamis* and the *energeia*, both the dynamic and the static, both the teleological and the mechanical, both the *Progression* and the *Equation*. Biological literature is nowadays full of the recognition of the concurrent presence in vital phenomena of the two modes of organization; and biologists are wondering whether any theory of the principle of life is possible which is not to commit suicide by cutting its throat on the ragged edge of facts of one or other of these two sorts. Yet we find indications in recent work of ways in which biologists of the two schools, vitalistic and mechanical, are finding points of common ground. The development of the statistical method is showing ways whereby the variations and progressions demanded by vital teleology and genetic process may be allowed, at the same time that exact formulas of distribution are applied to the same cases taken in the mass. In this way, for example, that bugbear to many advocates of purpose in the world, natural selection, becomes consistent with a teleological philosophy. Economists and criminologists are reaching exact statistical formulas for events involving individual desire and choice. It is beginning to be seen, even by the most positivistic science, that genetic processes may be developed in series which also allow statement in cross-sections and equations at each successive stage. All this means that the world is after all one, and that the categories of mental construction, derived in a process of evolution by actual treatment of the world, *cannot finally reflect processes in essential contradiction with each other.*

The needs of philosophical thought, however, are more urgent; and I have no desire to deny the impulses of consistent reflection which are impatient during the slow processes of evolution. Philosophy asks: How can we think reality in one thought? In the terms of our present discussion: How can body and mind, being what we have come to think them to be, live hospitably housed together in one phenomenal group of facts? In answer to this we have all the modes of reasoning of modern philosophy, from the consistent negative processes of abstrac-

tion of Hegel, to the logical 'rectification of concepts' of Herbart and Bradley. In opposition to both these tendencies, the present speaker holds that the category of final interpretation must be *a full one, not an empty one*, if it is to have concrete significance, and if it is to have the respect and win the adherence of that class of men who make actual contributions to knowledge — the men of science. It is to be sought in the interpretation of *the actual coefficients of the fullest reality of which we can have experience*. The realities of organic life are 'fuller' than those of inorganic nature, because they require just the differential genetic treatment which characterizes the distinction between the Equation and the Progression; because life exhibits ends as well as transformations. The mental life is a form of reality which in turn is fuller than the organic; because it yields the subjective, and requires again another genetic differentiation of experiences into the subjective and the external. If we go further, we find that the highest organization of the mental life is again the fullest, the richest in what we mean by reality in its complex modes. We go from spontaneity to effort, from presentation to reflection, from fact to ideal, from automatism to self-determination. The fullest, not the emptiest, the concrete experience, not the logical universal, is the point of view of most adequate interpretation, because it is just by the processes of expansion and growth that nature makes to us her revelation. We do not realize nature by abstracting from her content. When we speak of a final or absolute experience what we mean, if we mean anything worth while, is an *all-comprehensive and completely-full experience*.

Now — to state a point of view, not to expand or justify it — there is a type of mental organization which is in certain ways 'fuller' than any other, which requires and feeds upon — or to speak philosophically, 'transcends' — the opposition between fact, with its formulations in the Equations of positive science, on the one hand, and purposes, ends, values, and Progressions, on the other hand; it is what is commonly known as *Æsthetic experience*. In the essential union of the two points of view respectively of the 'producer' and the 'spectator' from which a work of art may be approached, we find in our experience the richer

whole. In æsthetic contemplation there is the fulfilment at once of the demands for a system of relationships essentially finished and formulated — something completely true — and also the satisfaction of a genetic ideal of perfect outcome — something divinely fair!

I should hesitate to state so bald a preference in philosophy, however, and then leave it entirely without justification, at least so far as defense against possible criticisms brought from the point of view of genesis is concerned; so I shall close with the statement of two considerations which tell with some force in favor of what I have elsewhere christened in advance 'Æsthetic Idealism.'

In the first place, if we recognize a genetic process in consciousness, we must be consistent in maintaining this or that genetic level without confusion. Failure to be consistent in this respect was our charge against both forms of the interaction theory. It follows that if consciousness goes on to a level in which the mind-body dualism fails, if consciousness itself seems to achieve a union of the terms of the earlier opposition, then it is a fair use of the genetic method to take this higher point of view. I think this is the case; that in æsthetic appreciation we reach a form of immediacy of experience in which the dualism of external and subjective is blurred and tends ideally, at least, to disappear.<sup>1</sup>

In the second place, the problem of validity, which in psychological terms is that of belief, sets the requirement that our final postulate be not, or not merely, a logical abstraction. Logical abstraction is well in its sphere, but through its abuse the whole distinction between mind and body, together with the network of further distinctions which confuse and balk us, have their origin. So far as philosophy — to use the terms employed above — can keep to the actual fulness of experience, as enriched with the gains of its genuine genetic constructions, so far is its understanding of reality also rich and real. Now it is, in my opinion, in the æsthetic category that such genuine constructions reach their fullest development and fruition. In æsthetic

<sup>1</sup> Cf. Ormond, 'Foundations of Knowledge,' Pt. II., Chaps. IX., X., for an interesting exposition of this higher immediacy of æsthetic consciousness.



satisfaction both the scientific impulse and the motor processes of practical life reach their 'end-state' — here they jointly 'issue and complete themselves.'<sup>1</sup>

If I should now add to the count of our catch given above (at the end of the third section of this lecture), I should hold up to view certain small fishes.

First, a method is employed which aims to be strictly genetic, and a symbolism is suggested which may be used in treating all of the problems of philosophy on which genetic analysis is able to throw light. In our fishing figure — this may be likened to a somewhat novel form of net.

Second, I think it has been shown that science demands psychophysical parallelism and is content with it. It may be said by some fishermen here that this is but a sorry flounder! Yes; yet the flounder is undergoing evolution, and he shows several very promising characters. The genetic character of science is being recognized, as equally marked with the quantitative character; and there is hope for a theory of 'correlation' of these characters, which will yield a higher adaptation in the whole realm of science. Psychological parallelism then is, from the point of view of science, our positive catch.

Second, as to philosophy; we have dragged certain monsters which we have had to throw away: Interaction in both its forms — the 'cause' theory and also the 'effect' theory of mind. Having thrown away these unprofitable forms, we have thought we felt the nibbles of a fish of another species named 'æsthetic,' a variation in the large family of Idealisms; but we have not dared to show him above water — admitting, indeed, that by a great effort we might be able to land him at all!

<sup>1</sup> See the writer's *Dict. of Philos.*, art. 'End-state.'

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES ROWLAND ANGELL.

I.

FATIGUE; SUGGESTIONS FOR A NEW METHOD OF INVESTIGATION.

BY C. R. SQUIRE.

A period of constructive work upon a scientific problem is usually followed by one of criticism. This period of criticism is, however, the precursor of another productive period. For after flaws in the apparatus or in the method of procedure have been brought to light the attempt to remedy them is likely to follow. When the concepts involved are clarified the problem is rendered capable of more definite statement.

We have reached the second period in the study of fatigue. The attempts of Mosso and Griesbach to establish simple and convenient psychophysical methods for the investigation of fatigue, because of their very clearness and conciseness of statement, have been most open to attack. Müller's<sup>1</sup> criticism of the ergograph, trenchant as it is, will not lead us to discard every form of this instrument, until a better one has been substituted. On the other hand the æsthesiometer will scarcely be considered again for an investigation of fatigue by one who is familiar with the literature on the subject.

There is, however, a numerous class of investigations that have met with scarcely any criticism: These may be classed together, since they attempt to measure fatigue in the gross by ability to add or perform some other associative act of greater or less complexity. Most of these studies have emanated directly from Kraepelin's laboratory or have adopted in some particular

<sup>1</sup> 'Ueber Mosso's Ergographen,' *Phil. Stud.*, XVII., 1-29.

the methods there used. Thorndike's<sup>1</sup> criticism was directed against certain dogmas that have arisen as a result of these investigations, rather than against their method of procedure.

This type of investigation appealed to educators. The method was simple; it could, apparently, be directly employed in the schoolroom upon masses of children. This apparent feasibility, together with the lack of wholesome criticism, gave rise to a considerable number of such studies. The total result is a series of pedagogical maxims as to the relative amount of fatigue induced by the various subjects on the school program, the proper alternation of work and rest, and other maxims of similar nature. Unfortunately these maxims rest upon a very insecure scientific basis. The method of procedure is so gross that the results are not susceptible of an unambiguous interpretation.

In a review of an investigation of this class made upon school children, MacDougall<sup>2</sup> makes the following pertinent criticism: "The test does not get at the phenomenon at all directly or unequivocally. The material from which the results are read is the product of the total set of mental conditions obtaining at the time of the investigation; and the number of errors in any given case will as readily be affected by a feeling of rivalry between the pupils or by a momentary distraction as by the influence of fatigue itself. These influences cannot unconditionally be set down as constant factors, which are therefore eliminable. The anticipation of recess or the conclusion of work may very well be potent in establishing a law of rhythmical increase or decrease in the number of errors, which will well combine with the actual exhaustion effects to produce a curve which does not at all truly represent the rise in fatigue."

The most obvious fallacies that have determined the method as well as the interpretation of results in the majority of these investigations are:

1. That muscular and central fatigue are interchangeable terms; that one can be directly expressed in terms of the other.
2. That fatigue arising from one kind of psychical activity

<sup>1</sup> 'Mental Fatigue,' *PSY. REV.*, VII., 466-482; 547-579.

<sup>2</sup> *PSY. REV.*, VI., p. 203.

can be measured by one involving totally different processes. This is one of the most frequent fallacies.

3. That incompetency due to fatigue, and decrease in interest due to the monotony of the task, are one and the same. Few of the investigators discriminate between them.

4. That the number of arithmetical operations, etc., performed during a given time represents the whole of the psychophysical activity, and consequently the percentage of decrease in the number can be regarded as function of increase in fatigue. Leuba<sup>1</sup> pointed this out in his criticism of the Griesbach method.

5. That an experiment can be framed which for an hour or more will be a direct correspondent to the same period spent in the ordinary activities of school.

6. That children are fit subjects for fatigue experiments.

Children are incapable of the prolonged disinterested attention demanded by the fatigue experiment. The only test which can ever be used in investigations upon school children with any hope of obtaining reliable results, must be of a similar nature to Griesbach's. Meumann<sup>2</sup> suggests the use of changes in blood pressure, pulse and respiration as measures of fatigue in such cases. A trustworthy test of this character is highly desirable.

However, it is not the purpose of this paper to criticise earlier investigations; but rather in the light of what has been done to offer suggestions for further study.

What must be demanded of the new method? First, that it make possible the separation of the component elements, fatigue of muscle and central fatigue, and that it select one of these for systematic study. Secondly, that it make use of a definite method of procedure, which shall be carefully regulated, so that all the conditions shall remain constant, or vary in known directions. Thirdly, that the measure and the process measured shall be comparable.

This, as previously stated, is very frequently overlooked in the formulation of fatigue experiments.

<sup>1</sup> 'On the Validity of the Æsthesiometric Method as a Measure of Mental Fatigue,' *PSY. REV.*, VI., pp. 573-598. Cf. also Germann, *ibid.*, p. 599.

<sup>2</sup> 'Entstehung und den Zielen der experimentellen Paedagogik, Deutsche Volksschule,' V.

## METHOD.

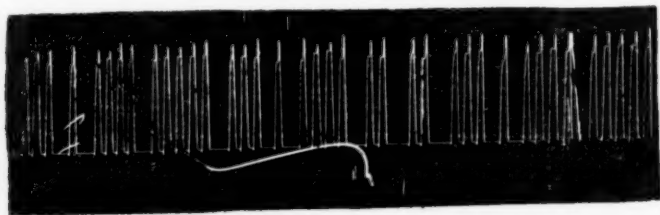
The method here described was devised to meet these requirements. It seeks to eliminate muscular fatigue in order to observe the rise and general course of fatigue of attention, exempt as far as possible from peripheral distractions. This, of course, necessitates a device that shall be fine enough to register the fluctuations in the attention during the course of the experiment.

A pattern of ten digits written upon a slip of paper was given to the subject, who was required to memorize it before the experiment began. The subject was then asked to tap the pattern given him for a certain length of time. By the use of the devices of tambour, smoked drum and time-marker all variations from the pattern and all changes in the rate of speed could be read at the conclusion of the experiment and paralleled with the introspective report of the subject.

The next consideration was a means of getting at any muscular fatigue that might be induced by the tapping. The ergograph was used for this purpose. The presumption was, that if a certain muscle or group of muscles could do as much work in a given unit of time immediately after the tapping as before it, the muscular fatigue in that particular case was a negligible quantity. This test would be valueless, of course, unless (*a*) the same muscles were used in raising the weight and in tapping, (*b*) the movements made were of the same character and in the same direction in both instances. Both of these requirements were met, as will be seen.

The reagent sat in a swivel chair, raised so that his left forearm rested comfortably in the prone position on the arm-rest of the ergograph. The second and fourth fingers were clamped to the board, and the third finger was enclosed in a padded metal splint for its entire length. By means of the splint all movements of the third finger both in tapping and in raising the weight were restricted to flexion and extension at the metacarpophalangeal joint. The forward and backward movement ordinarily used in raising the weight was converted by means of a system of pulleys devised by Prof. Angell into the up-and-down movement made in tapping. This fact was accidentally

verified. In one of the practice experiments, a disarrangement of the apparatus had delayed the experiment and disturbed the subject. When the experiment was finally begun, the reagent forgot that his finger was attached to the weight and tapped, as he thought, the pattern previously given him. The result was an ergographic tracing of the usual excursion arranged in groups. (A part of this tracing is given below.) A hook attached to the dorsal side of the finger piece that secured the splint and a corresponding S in the end of the wire most remote from the weight made it possible to pass quickly from the ergographic test to the tapping. During the tapping the splint and



finger piece were left exactly as they were when the weight was lifted.

The order of procedure was as follows: The pattern was first given to the subject; when he was confident that he had memorized it, the finger was attached to the weight. At a signal from the operator, the subject, his eyes being closed, began to lift the weight. He continued to do this until he received the signal from the operator to stop. This was given at the end of a minute. No restrictions were made as to the rate at which the weight should be lifted. It was thought that a compulsion to work at any other than the natural rhythm would of itself induce fatigue. The ergographic tracing, together with that of an electric time-marker, was made upon the smoked drum of a kymograph.

After the minimal pause necessary to detach the weight and adjust the lever for tapping, a third signal was given and the subject began to tap the pattern previously learned. The writing point of the time-marker was adjusted as closely as possible under the writing point from the tambour in order to economize drum space. The tapping, except in the practice series, was



continued until the drum was filled or time at disposal of the subject at an end. Some of the later series lasted a little over forty minutes.

The kymograph was then quickly adjusted for the second ergographic test. The lever from the tambour was set aside, weight attached, and the tracing taken for the same time and in similar manner to the first.

The objections that have been raised to the use of the ergograph in fatigue tests were either met by some arrangement of the apparatus or were naturally ruled out through the use we made of it. The leverage was kept constant. This was insured by the arrangement of the splint. Müller attacks the ergograph on the ground that it is impossible to isolate a single muscle or even a group of muscles with it and so study their fatigue. This does not vitiate our results, however, since no matter how many muscles or groups of muscles were active, the same muscles and the same groups of muscles were engaged when tapping as when lifting the weights.

In both the lifting and tapping we were able to discern a movement of muscles of the forearm. This would appear to substantiate Müller's statement.

Another objection that has been made to the ergograph by Müller and others with right is, that the necessary tightness of the band around the third finger hinders circulation and consequently induces fatigue earlier than it would otherwise arise. We were also able to rule this out in nearly every test. The pull of the weight which ordinarily tends to displace the finger piece when the movement is backward and forward, served in this case with the up-and-down movement to hold the finger piece in place. The bands were consequently kept much looser than would have been possible with the usual arrangement.

Moreover, the ergographic test was never continued to the fatigue point. We were not concerned with the curve of muscular fatigue nor with the absolute amount of work done, but with the relative amount before and after the tapping which constituted the fatigue test proper. In the first trial series thirty seconds was taken as a unit for the ergographic test; it was found, however, with the weight of 800 grams which was used through-

out the experiment, that one minute was not long enough, even when followed by the tapping, to induce muscular fatigue. (See table.) It was consequently taken as the unit in all the series that followed.

The tapping in the first trial experiments was continued for twenty minutes, although incipient fatigue was evidenced some time before the end of the twenty minute series; this time, however, was too short to give a fair expression of the course of fatigue. In some of the series the tapping was continued for forty minutes. It was impossible to regulate the time of tapping exactly and get the maximum duration, since we were not always equally successful in economizing drum space. One subject had but an hour at his disposal; when there was a delay in starting the experiment, it was frequently necessary to stop the tapping at the end of thirty minutes. In most cases fatigue of attention became evident after fifteen minutes.

Although all movements tend to become automatic, the experimenter had hoped that a pattern of ten digits would present difficulties enough to prevent its mechanical reproduction within the short space of time given to the experiment. Here a difficulty arose. One subject in particular manifested a very strong tendency to reduce the whole process to automatic action. To obviate this difficulty the subjects were asked to give the series in alternation with the series multiplied by two; on the next day the pattern given was varied in two additional series. The series were made more and more complex until the test consisted of the pattern and four variations. Then the order was reversed, with three, two and one variations, until finally the subjects were simply asked to tap the series memorized. The patterns were as follows:

July 7th	4 4 2 1 3 2 2 4 3 1 then
Series multiplied by two, or	8 8 4 2 6 4 4 8 6 2 then
Ser. multiplied by two minus one, or	7 7 3 1 5 3 3 7 5 1.
July 8th	3 2 2 4 3 2 4 4 2 3 then
Series minus one, or	2 1 1 3 2 1 3 3 1 2 then
Series multiplied by two minus two	4 2 2 6 4 2 6 6 2 4 then
Ser. multiplied by three minus three	6 3 3 9 6 3 9 9 3 6.

July 9th	1 1 4 2 1 3 2 2 1 3
Series plus two	3 3 6 4 3 5 4 4 3 5
Series multiplied by two plus one	3 3 9 5 3 7 5 5 3 7
Series multiplied by two plus three	5 5 11 7 5 9 7 7 5 9
Series multiplied by two minus one	1 1 7 3 1 5 3 3 1 5.
July 10th	3 1 2 1 4 3 4 1 2 1
Series multiplied by three	9 3 6 3 12 9 12 3 6 3
Series multiplied by three plus two	11 5 8 5 14 11 14 5 8 5
Series multiplied by two plus three	9 5 7 5 11 9 11 5 7 5.
July 11th	2 1 3 4 1 1 2 4 2 3
Series multiplied by three minus one	5 2 8 11 2 2 5 11 5 8
Series plus three	5 4 6 7 4 4 5 7 5 6.
July 12th	3 1 1 2 4 3 3 1 3 4
Series multiplied by two	6 2 2 4 8 6 6 2 6 8.
July 14th	2 4 2 1 2 4 1 3 3 2.

The subjects were: Miss H., a principal of a ward school in Chicago; Mr. B., a teacher of mathematics, and Mr. K., also a teacher of mathematics. None of these had previously had much practice in introspection. This lack of previous practice was, however, more than counterbalanced by their interest and the pains which they took to follow all directions given.

After five practice series had been taken, it was evident that the tendency to automatic action in two of the subjects would make it impossible to study the fatigue of attention if the simple pattern alone were reproduced. The course of procedure just described was then begun. The subjects had by this time become familiar with the apparatus and the general course of the experiment.

The hours of experimentation were: *B*, 8-9 a. m.; *H*, 10-11:15 a. m.; *K*, July 1 and 2, 3:30 to 4:30; after that 1:30-3.

In the time at our disposal we could only hope to demonstrate the feasibility of our method.

It now remains to be seen whether the results warrant the assumption that the method above described meets our requirements. Do the results indicate that it was possible to regulate the experiment so that we are able to disregard the element of muscular fatigue? The following table answers this question:

TABLE I.

	H.				B.				K.			
	Number.		Distance.		Number.		Distance.		Number.		Distance.	
	I.	II.	I.	II.	I.	I.	I.	II.	I.	II.	I.	II.
July 1	46	53	167	206	38	40	147	180	60	61	211	246*
" 14	42	49	147	187	39	42	152	174	54	60	225	220
" 2	50	54	100	108	40	37	137	142	51	68	214	227
" 12	40	47	175	186	35	42	140	164	52	61	246	284
" 7	37	48	116	133	36	36	181	206	63	48*	172	194
" 11	37	47	126	160	36	40	151	183	56	54*	212	270
" 8					34	42	178	228	64	56	280	265
" 10	38	45	156	148*	40	38*	150	164	45	49	196	205
" 9					35	37	133	151	53	53	175	154

This table records the number of times the weight was raised by each subject before (I.) as compared with the number of times after (II.) the fatigue experiment; the distances through which the weight of 800 grams was raised in the two instances are also compared. The results are grouped with reference to the complexity of the series. It can be seen that muscular fatigue was a negligible quantity in most of the series, for with a very few exceptions the weight was raised a *greater* number of times and through a greater distance after the fatigue experiment than before it. All of the subjects at one time or another reported that the excursion they would have made in the second test was cut short by the limitations of the apparatus. This difficulty was never experienced in the test preceding the fatigue experiment. The objective difference in ability to raise the weight in the two instances was thus really less than the subjective, as reported by the subjects. They found relief in the raising of the weight after the strained attention required in tapping. It was undoubtedly this feeling that brought about the constant difference in favor of the second test.

A decrease in the number of times the weight was raised is more frequent than a decrease in the distance through which it was raised. Kraepelin believes that the first factor is conditioned by central fatigue; the second alone by muscular. Our results do not substantiate this theory, however, since there was evidence of central fatigue, when there was a marked increase in the number of times the weight was raised.

The introspective reports of the subjects explained nearly all the exceptional cases.

These cases were found in nearly every instance to be due to some change in the conditions of the experiment, most frequently to a disarrangement of the apparatus. If we discard all those series—marked in the table with an asterisk—in which some disturbance in the regular conditions of the experiment was noted either by the subject or the observer, our results surely warrant the conclusion that this method gives us a means of eliminating muscular fatigue; and that the errors made in the production of the series tapped were not due to muscular fatigue. Our results seem to indicate that central fatigue does not decrease ability to do muscular work.

Thorndike<sup>1</sup> with the dynamometer could find no decided decrease of physical power after continued mental effort.

Bolton<sup>2</sup> found that "Certain forms of mental activity, probably such as are associated with *motor* excitations, appear to increase the height of the ergographic curve, although they undoubtedly produce central fatigue." He says again, "Dennoch beweisen unsere Versuche mit vollster Bestimmtheit dass es nicht angeht, aus dem Sinken der Muskelkraft irgend welche Schlüsse auf den Grad der geistigen Ermüdung zu ziehen, um so weniger als Oseretzkowsky auch nach dem sehr ermüdenden Auswendiglernen eine erhebliche Zunahme der Muskelleistung fand. Wahrscheinlich sind die gegenseitigen Beziehungen sehr verwickelte. Bevor also Massenversuche angestellt werden, ist es unumgänglich nothwendig, mit grösster Sorgfalt der Wechselwirkung zwischen geistiger Thätigkeit und Muskelleistung im einzelnen nachzugehen, damit wir erst ein richtiges Verständniss dafür gewinnen, was wir eigentlich messen."

We quote Bolton at length as he here emphasizes the necessity of a method of procedure similar to that followed in this experiment, *i. e.*, the elimination of one factor in order that we may more accurately measure the other.

It is, however, surprising that one is capable of such prolonged and fairly rapid contraction of the muscles of the finger as is necessitated in the tapping of these patterns for forty minutes, without inducing well-marked muscular fatigue.

<sup>1</sup> 'Mental Fatigue,' *PSY. REV.*, VII., 578.

<sup>2</sup> 'Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut und Muskelleistung,' *Psy. Arb.*, IV., 187, 232.

Take one record, that of *B* on July 14, for illustration. We find that the pattern was repeated one hundred and twenty-three times before the close of the series. The pattern when completed requires twenty taps. If we neglect those taps that were repeated and omitted—the one will probably nearly balance the other—the total number of taps made by *B* in that record was 2,460. This is by no means the greatest number of taps made during any one series, for the complex patterns required a greater number of taps for the complete pattern and were frequently repeated quite as many times as the one taken for illustration. When we turn to the table, it is seen that there is no evidence of muscular fatigue in the ergographic record taken after this tapping.

Moore<sup>1</sup> says that he once made 480 taps, although he was sure that he could not have made one hundred more at the close of any record. In his records fatigue was manifested as early as the seventieth tap. The early rise of fatigue in his case may have been partly due to the arrangement of the apparatus; the tapping may have been violent and at a speed exceeding the natural rate of the subject. (While it is not the aim of this paper to discuss the question of the rise of muscular fatigue, the query suggests itself whether tapping could not be prolonged indefinitely, if circulation were unimpeded and the natural rate of the subject maintained, without giving rise to muscular fatigue.)

The record of *H* for July 2 is free from any trace of fatigue. Her rate was very slow; she followed the rhythm of the time-marker, one tap a second. At this rate she was able to give series after series without making an error. The rate here is much slower than that of the other subjects and also than that of her own subsequent records. In her introspective report she notes that this series was not so fatiguing as that of the day previous; because of its variety it was more interesting and therefore easier to attend to.

Is not the early rise of fatigue in Moore's experiment due in part to the direction of the attention upon the contraction of the finger and the sensations involved? In our problem the atten-

<sup>1</sup> 'Studies of Fatigue,' *Studies from Yale Laboratory*, III., 92.



tion was necessarily directed upon the pattern and the associations called for. Is it possible that the central excitation retarded the rise of muscular fatigue? However that may be, muscular fatigue did not play a noticeable part in these experiments and can be disregarded when we attempt to explain the irregular and inaccurate tapping.

It will surely be apparent at once that our second requisite of a satisfactory method was fully met; viz., that it was possible to regulate the method of procedure so that the conditions should remain constant or vary in known directions.

It can also be seen from the description of the method that the measure and the processes measured are comparable, since by the terms of the experiment the one is contained in the other. The measure of fatigue was the increase in the number and duration of errors and irregularities during the production of the memorized and associated series.

The errors and irregularities were of various sorts; taps added or omitted, groups added or omitted, groups broken by long pauses, whole groups or series repeated, irregular succession of groups in which there was no trace of the pattern given and at times the whole series of ten digits was omitted.

None of these inaccuracies or irregularities as such can be considered indices of fatigue; lack of practice would also cause irregularities; but irregularities due to this cause would *decrease* as the series were repeated. Irregular following upon a long period of fairly correct tapping, *i. e.*, after practice has had its maximum effect, can hardly be regarded otherwise than as a pure fatigue phenomenon, unless it be due to lack of interest and consequent inattention. Lack of interest could not be advanced as an explanation in the case of these subjects.

As fatigue enters, recall is halting and often fails, the associations weaken. As the associations fail, some appear to hold together better than others.

K in a certain series had firmly associated the combinations 5, 8, 5 and 3, 6, 3. As he became more and more fatigued, he interspersed these with groups from all the series regardless of order of groups or series. Again, the first five or six groups of the first series were more closely associated than the last mem-

bers of this series; so were the last five or six groups of the second series, than the first members of that series. As the experiment was continued and fatigue became apparent, he frequently omitted the intervening groups, although he never failed to give these in their correct order. This is doubtless explained by the fact that all of the subjects tended to divide the series of ten digits into smaller series. Thus the groups memorized together were undoubtedly recalled together.

The results show that:

1. Gross irregularities become more frequent as the series are continued.
2. The increase in inaccuracy and irregularity is not continuously progressive, but shows fluctuations.
3. While the complex series, as we should naturally expect, show a greater number of errors at the beginning of the record than the simple series, the proportion of increase as the record proceeds is really less than in the case of the simple series.

Certain of the complex series have not only a relative advantage over the simple with regard to the increase in inaccuracy, but an absolute. The number of errors was least for *B* when one variation of the original series was introduced; for *H* and *K*, when the original series had two variations added.

Scripture<sup>1</sup> found that "The fatigue in reaction time increased with the complexity of the adjustments required for perceiving the stimulus. There is least fatigue where only an effort of attention is involved, more when the act of accommodation is added and still more when the act of convergence is added."

The two instances, though they might appear comparable at first sight, are not. An experiment in reaction time by the very conditions of the experiment keeps the attention on the alert; each stimulus gives a new impulse. With the tapping the control was central; no external stimulus served to reinforce the central mandate. Therefore, the greater the monotony, the greater the strain on the attention and the more fatiguing the series.

*H* says: "At times the deadly monotony of the tapping

<sup>1</sup> 'Researches in Reaction Time,' *Yale Studies*, IV., 12.

nearly overcomes me." At such times peripheral stimuli tend to force themselves upon the attention and lapses occur.

The most complex series were also extremely fatiguing; the associations were not so readily made; there was also a greater number of things to be memorized. When the pattern was *slightly* varied, the demands upon the attention were not so great as with the simple or very complex series.

The effectiveness of the simple as compared with the complex series in producing fatigue, varied with the individuals. *H* always found the simple series very fatiguing because of their monotony; while *B* says: "The simple series produce less fatigue. They become in part automatic."

With the most complex series the subjects frequently reported a feeling of relief as they returned to the original pattern. One variation of the original series was at times apparently easier to give than another just preceding it or following it. This might very well be expected, since more associations were required in some cases than in others, etc. To double a series would naturally be less difficult than to double and then to subtract one.

A general periodicity in the number of errors was to be observed. A similar phenomenon was frequently noted in the height of the excursion of the writing point. Such records began with a maximal excursion, which seldom lasted beyond the second rotation of the drum, a period of about five minutes' duration. After this the excursion grew smaller quite rapidly. Then followed frequent fluctuations in the height of the excursion, although the maximal height was never reached again.

The more complex the series, the less marked was the rhythm above noted. In these series the excursion throughout the record was slight and quite even. This phenomenon was not frequent enough to make a detailed study of it possible.

However, it is possible by means of the irregularities in the tapping to follow the general course of the fluctuations in the attention.

The facts brought out by the records are:

1. The duration of the fluctuations increases as the series progress.

2. Their frequency increases also, although not continuously.

3. Their duration and frequency are less in those complex series which give the advantage of variety without calling for too many and too involved associations.

A period of prolonged inattention or inaccurate and irregular tapping is always followed by one fairly free from fluctuations. Kraepelin<sup>1</sup> and Voss<sup>2</sup> have noted the same phenomenon and have given it the name of '*Müdigkeitsantrieb*.' This is especially noticeable in *K*'s record.

It was impossible to rule out all distractions arising from external stimuli. Those most frequently noted were:

1. Click of the time-marker, strong tendency to follow its rhythm especially in the early series. "After the first tap there was a tendency to wait for the time-marker to click before making the next tap. This gave an opportunity for the attention to wander, especially toward the sound of the marker, and often resulted disastrously."

2. The sensations resulting from the impeded circulation consequent upon the binding of arm and finger.

3. Constrained position of the body. *H* resented the fact that she was obliged to close her eyes. This resulted in a distraction of the attention.

4. The pounding of the workmen outside, mentioned only by *H*.

5. The click of the metal finger piece, which was heard when the splint became disarranged, served at times as a distracter, at other times as a stimulator.

The fluctuations, however, were not dependent upon external causes. Neither were they subject to voluntary control, as the following report shows: "Could anticipate lapses of attention but not ward them off. The feeling was not similar to that experienced in muscular fatigue. Merely a sense of the inevitableness of the impending lapse. When the lapse came, it was not recognized as such until after the mistake had been made. This, of course, called forth effort to prevent further

<sup>1</sup> 'Ueber Ermüdung und Erholung,' *Psy. Arb.*, I., 678.

<sup>2</sup> 'Ueber die Schwankungen der geistigen Arbeitsleistung,' *Psy. Arb.*, II., 409.

blunders, which were generally prevented, but a sense of strain was always felt before the work would run smoothly again. Moments of especial clearness were observed when I could remember the series for quite a distance ahead. These may be the crests of the attention wave. They were not subject to control."

These are not the normal fluctuations of attention, which are present even in the most intense attentive state. Several important differences in the normal and the fatigued attention wave are to be noted. (1) The normal fluctuations are much shorter in duration. (2) The normal are more regular in their order of occurrence. It is only necessary to observe the records near the close of a long and difficult series to note the great irregularity of the fluctuations in a state of fatigue. (3) The lapses are more intense and recovery more difficult in the fatigued state. All of the subjects report occasions of total blankness. *K* experienced them most frequently as times when he is 'entirely at sea as to which of the series was being tapped.' *B* says: "Sometimes I have felt completely lost, utterly in the dark for an instant. Recovery in this case, although rapid, was with unusual effort." (4) When the subject is fatigued, voluntary control is weaker; he is not able to shut out distractions that would not trouble him ordinarily. The simple series gave more occasion for fluctuations of this nature than did the complex. The ease of the series and the monotony of the task favor mind-wandering in the most conscientious subject. *K* compares this state to that experienced when fatigued by adding long columns of figures. With the complex series, peripheral distractions are not so frequent a cause of fluctuations. In these series, lapses are due to difficulty in making proper associations and to halting recall of groups and of series. "The interval which was required to decide what series should come next was often long."

What is the cause of these fluctuations? It is most probable that they have much in common with fluctuations of attention under normal conditions. In case of fatigue, however, inner-  
vation is accomplished with greater difficulty and is therefore accompanied by sensations of bodily strain or a 'feeling of

effort' as reported by the subjects. Thus the period of uncertainty is lengthened and an external stimulus such as the click of the apparatus may serve either to enforce or distract, according to the disposition of the individual.

A decrease in rate of speed has been regarded as indicative of fatigue; some experimenters have used it alone as a measure of fatigue.

Gilbert<sup>1</sup> in his experiments upon school children found that the loss of speed in tapping for 45 sec. at six years was 21.4 per cent.; at fifteen years 12.7 per cent.

Moore<sup>2</sup> found that fatigue lengthened the time, although the increase was not a steady one but showed fluctuations. Oehrn<sup>3</sup> repeats the statement frequently made by Kraepelin. "Two factors influence the duration of psychical processes. Practice produces an increase in their rate; fatigue a decrease." Scripture<sup>4</sup> found in his experiment on reaction time that a lengthening of the average times appeared sooner than an increase in their mean variations.

We were at first inclined to accept the current opinion, but our results, although most certainly indicative of fatigue, do not show any regular decrease in the rate of tapping.

The series were divided into five-minute sections, and the average duration of each of the patterns during the successive five-minute periods was found. Often there was an increase in the rate of speed as the tapping continued, although there was well-marked fatigue. On the other hand, while the size of the M. V. decreased for a time with practice, it gradually increased toward the close of the series.

It is quite as likely that decrease in rate of speed is due to a feeling of weariness, of tediousness. As that is an index of fatigue, the first is often confused with the latter. While this decrease of rate may be expressive of fatigue, it is not always and necessarily so. A certain irregular and fitful tapping at increased rate is quite as much an index as decreased speed.

<sup>1</sup> 'Mental and Physical Development of School Children,' *Yale Studies*, II., p. 40.

<sup>2</sup> *Op. cit.*, p. 95.

<sup>3</sup> *Psy. Arb.*, I., p. 92.

<sup>4</sup> *Op. cit.*, p. 19.



Some of the testimony clearly cited is in perfect harmony with this presumption. Gilbert found the decrease in speed greatest in the case of the young children who are less able to resist a feeling of tediousness, and less capable of a prolonged voluntary effort, than are adults. Our subjects were unusually free from such an influence; interest in the problem kept them at a point of the highest possible efficiency.

In our opinion the size of the M. V. is a much more accurate measure of fatigue than the average rate of speed. It can be regarded as the coefficient of irregularity, and together with the purely qualitative measure — the degree of accuracy — gives us a fairly reliable standard, when the method of experimentation does not take into account the duration and frequency of the fluctuations of attention, which after all furnishes the most accurate measure.

In our previous discussion we have taken it for granted that there is a state of fatigue, a condition of real incompetency. The fatigue induced by these experiments was not always general, or marked by incompetency in *all* activities of the day, during the hours that succeeded the experiment.

While the possibility of a state of general fatigue is conceded by nearly all investigators, there does not seem to be any recognition of the fact that real fatigue may exist without being general; that there is a state of special fatigue.

Kraepelin<sup>1</sup> distinguishes between '*Müdigkeit*' and '*Ermüdung*.' MacDougall<sup>2</sup> distinguishes between 'weariness' — personal and fluctuating and dependent upon a superficial act of attention — and 'fatigue' — a deep-seated nervous phenomenon. These classifications are helpful in calling attention to the fact that all which passes under the name of fatigue is not genuine fatigue, but is often a feeling of boredom with a distasteful task. However, if any piece of work is continued long enough, there arises a condition which we may well call special fatigue or real inability to continue the special activity at anything like the highest possible efficiency. It is a state of nervous exhaustion, though not so wide spread as when fatigue is general. Possibly a totally different activity could be engaged in directly

<sup>1</sup> *Op. cit.*, p. 623.

<sup>2</sup> *Op. cit.*, p. 203.

after the subject had experienced special fatigue without a noticeable failure in either quality or quantity of work.

Thorndike errs in failing to recognize special fatigue. We cannot measure ability for one kind of work by a test which involves other mental processes. It is not surprising, when we consider this fact, that Thorndike found school children were able to do as good work after school as before. "Why I do less work of some special sort after being engaged in it for a long time is not that I am not able but because I don't feel like it," is only partially true. If the work were strenuously continued for any considerable length of time, there would be real inability to do that *special* work effectively; although there might not be a decrease in the ability to do other work, which would be the case if the fatigue were general.

This method enables us to distinguish between general and special fatigue. It should also be possible by gradually lengthening the series to ascertain for each of the individuals the point at which special fatigue became general.

In these experiments we have been chiefly concerned to find a satisfactory method for the study of the fatigue of attention. The method could be used, however, to investigate the effect of fatigue upon recall. As we used it, it is not adapted to the investigation of the effect of fatigue upon the accuracy and readiness with which associations can be made. We found that the variations of the original pattern after a few repetitions were memorized as series; the associations or arithmetical operations called for by the terms of the experiment were omitted and the reproduction of the series became simply a matter of recall. However, the relation of fatigue to association can be investigated, as will be seen, by a slight alteration in the original method.

By a modification of the experiment, it would be possible to determine the effect of fatigue upon the perception and the time interval between stimulus and perception. Muscular fatigue might be eliminated as it has been in this experiment. In place of a memorized series the subject could be required to repeat the stimulus given by the experimenter, or still better by some apparatus arranged to give any desired number of stimuli in any desired order of succession. The fatigue experiment in this

case would be one in simple reaction time carried to the fatigue point and beyond.

This modification, with another slight variation in the method of procedure, would enable us to study the effect of fatigue upon the ease and accuracy with which associations are made. It would simply be necessary to require the reagent to make some definite associative reaction instead of giving a repetition of the stimulus. The ergograph furnishes us with a fairly reliable means of eliminating muscular fatigue in any of these cases.

## II.

### MEANING IN MEMORY AND IN ATTENTION.

BY KATE GORDON.

#### I. MEMORY AS DEPENDENT UPON THE COMPLEXITY OF ITS CONTENT.

It would require no small degree of skill and patience to give an adequate summary of all that has been well done and well said upon the subject of memory and its laws. Without attempting that, we may take it for granted that one or two tendencies are sufficiently apparent. In classifying the laws of memory it has generally been held, by recent writers at least, that there are, in the last analysis, two mutually irreducible principles of association. One principle stands for a connection between things whose first conjunction was fortuitous and whose bond is purely mechanical; the second represents a connection between objects which seem peculiarly fit to go together or which have some intrinsic relation quite apart from simple contiguity. In Wundt,<sup>1</sup> for example, we find the following: "Fasst man so \* \* \* alle Associationen als Resultanten elementarer Verbindungsprozesse zwischen einfachen Empfindungen oder relativ beschränkten Empfindungscomplexen auf, so sind nun an und für sich nur zwei solche Elementarprozesse möglich," \* \* \* *i. e.*, \* \* \* die Verbindung gleicher Elemente, und die Verbindung solcher, die durch gemeinsames Vorkommen in einen

<sup>1</sup> Grundzüge d. Phys. Psy., vol. 2, p. 468.

functionellen Zusammenhang getreten sind. Wir wollen diese beiden Formen der Elementarverbindung die Gleichheitsverbindung und die Berührungsverbindung nennen." These two forms are elsewhere called intensive and extensive, or internal and external association. Aschaffenburg<sup>1</sup> quotes in his work a number of proposed classifications which adhere to this same twofold division, for example, that of Trautscholdt as follows:

I. *External Association* (i. e., by contiguity).

- A. Of simultaneous ideas.
  - i.—2.—etc. (subordinate divisions).
- B. Of successive ideas.
  - i.—2.—etc. (subordinate divisions).

II. *Internal Association* (by similarity).

In the other schemes of division which Aschaffenburg quotes (Kraepelin, Wahle, *et al.*) and in his own, the distinction of inner and outer association is maintained apparently as ultimate. The reaction against the extreme form of associationism as held by Mill, Bain and others is represented in Stout<sup>2</sup> by the great importance which he attaches to the *conative unity* of consciousness. Stout also seems to uphold the classification of associations as external and internal. He calls them respectively the 'general unity and continuity of consciousness' and the 'conative unity and continuity of consciousness,' meaning, apparently, accidental and intrinsic connections between psychic elements. In Mr. James's categories of frequency, recency, vividness and emotional congruity,<sup>3</sup> we recognize, in the frequency and recency, the external or mechanical conditions of remembrance, and in the vividness and congruity, the intrinsic conditions which stand for the amount of attention and interest bestowed. James's analysis of the 'similarity' association, however, tends to reduce the usual sharpness of the difference between contiguity and similarity, inasmuch as he points out that similarity is a case of partial identity, which brings it back to terms of the contiguity of elements.

<sup>1</sup> 'Experimentelle Studien u. Associationen,' *Psych. Arbeiten*, Vol. I., p. 209.

<sup>2</sup> 'Manual of Psy.,' pp. 71 ff.

<sup>3</sup> 'Psychology,' Vol. I., p. 577.

If we accept the division of associations into external and internal, we may say that the greater part of the experimental work in memory has been engaged with the external or mechanical factor. The experiments of Wundt and Ebbinghaus, for instance, deal mainly with the influence of repetition and of time-intervals upon memory. The question in this type of experiment seems to be this: given a certain content, by what device can it best be impressed? In the experiments reported below the object was to study the nature of the content itself as affecting association, to find out what sort of material was impressive in itself. If we may say that the essential fact about memory is that earlier conscious process functions by representation in later conscious process, then we may call the test of effectiveness or goodness of memory the likelihood of recall which a given content enjoys. The question here at issue is: what effect, if any, does the value of a psychical object have upon its chances of reproduction? The experiments were devised under the assumption that meaning or value is to be defined on the functional side in terms of attention and interest, and on the structural side in terms of complexity; that the more complex a content is, the greater the numerical aggregation of elements, *i. e.*, the greater the number of its differentia, the more it means.

Among the writers who have recognized the quality of the object as a factor in association are the following. Külpe<sup>1</sup> says, in his chapter entitled 'Incentives to Reproduction and Liability of Reproduction,' that the liability of reproduction depends upon the nature of the conjunction of sensations in consciousness. Besides spatial and temporal colligation, he there mentions as important (a) the remoteness of impressions from each other in space and time. "The greatest liability of reproduction is correlated with a direct contiguity or succession." (b) The more a content differs from its surroundings, the easier will be unification and recall. (c) The existence of a name for the complex makes it more effective for central excitation.<sup>2</sup>

<sup>1</sup> Külpe, 'Outlines of Psy.,' pp. 199-202.

<sup>2</sup> Cf. also Lehmann, *Phil. Studien*, Vol. V., p. 135 and F. Angell, *Phil. Stud.*, Vol. XIX., p. 1.

He says, further, that the more individual the quality of the connected content, the stronger is the liability of reproduction. "The simple sensation, which \* \* \* can occur in the most various connections, does not as a rule possess any considerable effectiveness for central excitation. But more complicated processes, ideas, are often quite individual in character, etc." Kennedy<sup>1</sup> writes: "The accuracy of recollection is largely conditioned by the character of the content to be remembered." There are [he is summarizing the doctrine of others] two general ways in which a thing may be remembered. (1) By immediate recall, as when fine shades of color are reproduced just as they were in reality. (2) By mediate recall, in which some third factor enters. This is the way in which complex material is remembered; it is fixed by our concepts or classifications of it (*i. e.*, by translations or indirect means). Attention, repetition, rhythm and the general character of the object—these, he holds, govern the 'reception of the object.' "In general we may say that the reception which an object gets into consciousness depends very largely upon the sort of object it is." The idea expressed by the two men just quoted is, of course, commonly remarked, but experimental evidence has been rather more meager.

A research was conducted by Binet and Henri<sup>2</sup> upon 'La mémoire des mots' and 'La mémoire des phrases.' The authors here distinguished the memory of sensations from the memory of ideas. They used as content to be recalled, first, lists of figures (memory of sensations), then lists of isolated words, and finally words in rational combination, *i. e.*, sentences (memory of ideas). They found that words were recollected more easily than figures, and that among such isolated words those were most likely to be recalled which were most intimately connected with the subject's own habits. Sentences were better remembered than an equal number of disconnected words. It should be remarked that although Binet and Henri were working in this last research upon the memory of *ideas*, they appeared to make the exact verbal reproduction the test of correct

<sup>1</sup> 'On the Experimental Investigation of Memory,' *PSY. REV.*, Vol. 5, 483, 488.

<sup>2</sup> *L'Année Psy.*, Vol. 1, 1-23, 24 and 25.



memorizing; substituted words or equivalent expressions were not accepted as being equally good as the precise phraseology of the original material. Miss Calkins<sup>1</sup> and Mr. Kirkpatrick<sup>2</sup> have severally made experiments comparing the memorizing of the abstract (*e. g.*, of words) with that of the concrete (as of pictures). They reached the same general conclusion, namely, that 'the superiority of concrete (objects) to verbal, as memory material, is under-estimated rather than over-estimated even by its most strenuous upholders.' Particularly was this true in the case of delayed as distinguished from immediate recall. W. G. Smith<sup>3</sup> has studied the relation of attention to memory. Smith's experiments illustrate the effects of the distraction of attention from the memory process. Various forms of distraction were used which proved progressively engrossing, with the result that memorizing became poor in proportion as attention was withdrawn. Smith's general contention is that contiguity is a merely formal factor and that the real explanation of effective memory lies in attention and interest. Bigham and Münsterberg<sup>4</sup> have made investigations (with which those of Cohn<sup>5</sup> also agree) showing that the memory of words is more certain if they may be both seen and heard by the subject—that the apprehension by several avenues is better than by one. Miss Calkins in another investigation<sup>6</sup> says that the readily associated objects are the 'interesting' ones; and interest she analyzes (in agreement with James) into the primacy, the frequency, the recency and the vividness of the content presented. The outcome of her work shows that the material which does sustain these relations is that which is best remembered. Margaret Keiver Smith<sup>7</sup> in a study of 'Rhythmus und Arbeit' has shown the effect of variety in rhythm upon association. Nonsense-syllables were learned by heart, the number of necessary repetitions being the test of readiness of memory. It was found

<sup>1</sup> Calkins, *PSY. REV.*, Vol. 5, 451.

<sup>2</sup> Kirkpatrick, *PSY. REV.*, Vol. 1, 602.

<sup>3</sup> 'The Relation of Attention to Memory,' *Mind*, N. S., Vol. 4, 47.

<sup>4</sup> *PSY. REV.*, Vol. 1, 34.

<sup>5</sup> *Zeits. f. Psy. u. Phys. d. Sinnesorgan.*, Vol. 15, p. 161 ff.

<sup>6</sup> *PSY. REV.*, Vol. 3, p. 32.

<sup>7</sup> *Phil. Studien*, Vol. 16, p. 197 ff.

that, other things equal, the syllables which were presented in simple rhythms were harder to acquire than those which came in the more complex rhythms. Thus, syllables were learned more readily in the dactylic than in the iambic meter. Allin<sup>1</sup>

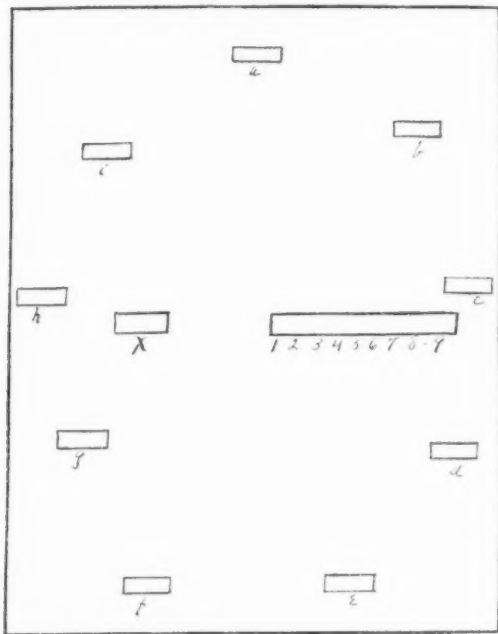


FIG. 1.

*S* represents a black screen 22 by 28 inches in which various small openings were cut and behind which a revolving wheel carried the printed syllables.

Case I. The syllables appeared one after another at *x*.

Case II. Syllables were shown one after another along the line 1-2-3-4.

Case III. They appeared along the same slot but in a haphazard order, *e. g.*, 5-3-1-9-6-8-7-4-2. This order would remain constant during the learning of any one series of syllables, but was changed every time a new series was begun.

Case IV. Syllables were shown in regular succession around the circle *a-b-c-d*.

Case V. They appeared in irregular succession, as *e. g.*, *f-g-c-e-h-i-a-b-d*.

is also an advocate of the general standpoint which grounds effectiveness in association upon attention and interest.

It may be in place to mention here one or two opinions on the nature of the memorizing of a complex content. Kennedy, as above quoted, distinguishes mediate and immediate recollec-

<sup>1</sup> 'Grundprincip. d. Association.'

tion, and says that the memory of complexes is mediated by some third factor intervening between the two primary members of an association. Scripture<sup>1</sup> and others allow the legitimacy of such a description and have shown that association between two objects may take place by means of a third object which is not consciously recognized by the subject as having performed that office. Cordes,<sup>2</sup> however, explains 'mittelbare Associationen' as cases in which the A-phenomenon (or first member) calls up as a B-phenomenon (or second member) a complex idea. The part which is afterwards analyzed out and called the third or mediating term, is, he says, just as much a part of the B-phenomenon as is the element which stood in the more prominent place in consciousness.

The general plan of the following experiment was to select the simplest feasible material for memorizing and to compare the time taken to learn this with the time needed to learn a content into which two or three complications upon the simple first form had been introduced. The first member in each comparison was a series of nine nonsense-syllables, *e. g.*, vax, tir, etc., printed on slips of white paper and shown one at a time through the same opening in a black screen, *x*, Fig. 1. The complication of this situation took three forms.

#### I.

In the first form of the experiment the content was complicated by adding to it a spatial differentiation. The list of nonsense-syllables, instead of appearing in the same place, *x*, appeared not only in temporal succession but also in spatial succession. The nature of this displacement was varied so that there were in all five different cases to be compared.

Each series was repeatedly shown to the subject until it was learned by heart. Thus the number of repetitions stood as the test of the difficulty of a series. After an interval of three days the same five series were learned over again in the same way, thus giving a test of their effectiveness in delayed recall. In order to distribute evenly the effects of fatigue and of practice

<sup>1</sup> 'The New Psychology,' pp. 201-202.

<sup>2</sup> Cordes, 'Ex. Untersuchungen,' *Phil. Stud.*, Vol. 17, pp. 73-77.

during a sitting, the five sorts of series were presented from week to week in revolving order: *e. g.*, (1) I., II., III., IV., V. (2) II., III., IV., V., I., etc. In the subjoined tables the Roman numbers stand for the five kinds of space relation, the Arabic numbers represent the repetition necessary for correct reproduction. Each of these numbers is the average from five tests.

## FIRST LEARNING.

I.	A.		B.		C.		D.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	4.75	.75	5.4	.72	4.2	1.04	7.0	2.00
II.	4.25	.75	4.6	.88	4.6	.48	8.4	2.08
III.	4.50	1.00	5.2	.64	5.4	.88	7.8	2.56
IV.	4.75	.375	4.0	.40	4.2	.84	8.4	.72
V.	6.50	1.50	4.8	.64	4.2	.64	8.0	1.60

## SECOND LEARNING—THREE DAYS AFTER.

I.	3.00	.50	2.8	.64	3.0	.40	3.6	.72
II.	3.75	.375	3.2	.64	3.0	.40	4.2	1.44
III.	3.50	1.00	3.6	.48	3.6	.48	4.4	1.68
IV.	2.50	.50	3.0	.40	3.0	.40	4.4	1.16
V.	3.75	.75	4.0	.80	3.2	.96	6.0	1.60

	E.		F.		G.		H.		I.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	5.2	1.04	6.2	.84	15.0	2.80	11.5	.87	5.2	.64
II.	4.4	.64	5.6	.72	11.00	2.0	9.0	3.50	4.6	1.52
III.	4.8	.96	8.0	2.00	16.00	5.2	10.5	2.00	4.2	1.04
IV.	4.0	.80	6.0	1.20	18.40	4.88	8.0	.50	4.0	1.20
V.	7.2	1.84	8.4	1.68	17.6	2.48	11.0	2.00	4.0	.80
I.	3.8	1.04	4.8	.64	9.2	2.56	4.75	.37	3.0	.40
II.	3.0	.40	4.0	.80	7.0	2.80	3.75	1.12	2.6	.48
III.	3.6	.72	4.4	.88	6.4	.84	5.00	1.00	3.0	.40
IV.	3.0	.40	4.4	.88	7.4	2.08	3.75	.75	2.6	.48
V.	4.2	.64	5.4	.88	7.8	1.04	5.75	1.75	3.4	.72

	B'.		E'.		F'.		General Average of all the Subjects.
I.	3.8	.64	5.6	1.52	5.4	.72	7.05
II.	3.8	.96	3.8	.64	4.6	.48	6.14
III.	4.0	.80	4.0	.40	5.2	.32	7.11
IV.	3.2	.64	3.2	.32	4.0	0	6.60
V.	4.2	.64	5.8	1.04	5.6	1.28	7.70
I.	2.6	.48	2.6	.72	3.4	.48	4.06
II.	2.8	.44	3.6	.48	3.2	.32	3.80
III.	3.0	.40	2.8	.64	4.2	.72	4.08
IV.	2.6	.48	2.2	.32	2.8	.32	3.63
V.	2.8	.44	2.8	.64	4.0	.80	4.61

The capitals A, B, C, etc., stand for the different subjects. The right hand column of figures under each subject shows his average variation.

The tables lettered B', E', F' represent a second series of experiments performed with B, E, and F, respectively. They show that the same tendencies which were apparent after five weeks' practice also persist at the end of ten weeks. The above data are not, of course, extended enough to warrant very detailed conclusions, but they point on the whole toward the uniform preference for the two forms which offer both complexity and regularity. The fourth form, which gives the best combination of individuality with unity, was easiest for nearly every person. The introspective evidence of the subjects agreed very closely with their results. Without any suggestion from the operator the following points were volunteered: that the fourth form was easier because each syllable had a place of its own: that it was pleasanter to anticipate a series in that arrangement than to keep the eyes fixed on one point as in the first form.

## 2.

The second part of the experiment was arranged to compare (I.) the learning of syllables printed in black letters upon white paper with (II.) the learning of syllables printed in black but each upon a slip of different-colored paper. Thus a series

FIRST LEARNING.

	J.		K.		L.		M.		N.		General Average.
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I.	5.9	2.30	6.75	1.25	5.4	1.28	8.33	1.77	3.83	.83	6.04
II.	5.3	1.22	6.25	.87	4.4	1.52	8.33	1.33	4.16	.83	5.62

SECOND LEARNING—THREE DAYS LATER.

	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I.	2.7	.56	4.75	.75	2.6	.48	4.33	1.00	2.66	.50	3.40
II.	2.3	.62	4.00	.50	2.0	.0	3.16	.80	2.50	.50	2.79

would consist of *vax* on a blue slip, *sed* on yellow, *ver* on rose, etc. No color was ever associated with more than one syllable. The subject was not required to remember or even to notice the colors — they were simply there to help or hinder as they could.

The syllables in both these cases, I. and II., were shown in temporal succession but without spatial displacement.

Thus four out of five did their best work with the colored backgrounds. Here too the subjective evidence agrees. In several instances the color was definitely ascribed as the cue by which the syllable was recalled.

## 3.

In the third form of the experiment, the simple series I. was contrasted with a series II., which combined both space and color differences. The first set was shown at *x*, Fig. 2, and the second set down the line 1-2-3-4.

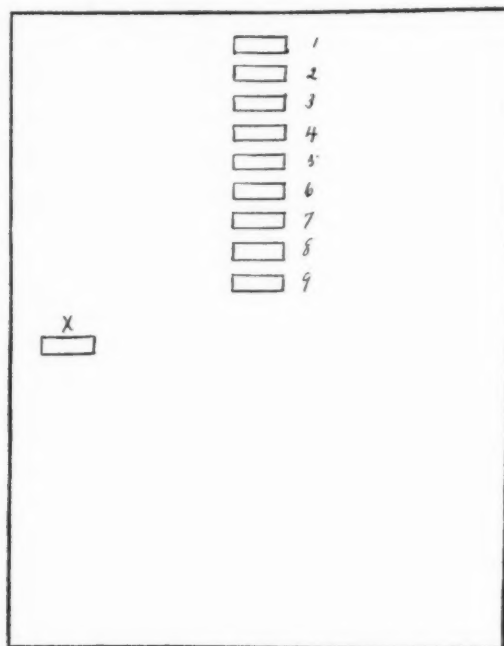


FIG. 2.

The results were as follows :

## FIRST LEARNING.

I.	O.		P.		Q.		R.		S.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	15.0	2.8	3.6	.48	3.16	.55	7.0	2.00	3.83	.55
II.	12.4	2.48	3.2	.32	4.16	.28	6.2	1.44	3.83	.55



## SECOND LEARNING.

I.	11.0	1.2	2.6	.88	2.33	.55	4.6	.88	1.66	.53
II.	9.2	4.6	1.6	.48	2.33	.42	4.2	.64	1.16	.33

	T.		U.		V.		W.		X.		General Average.
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I.	4.33	.66	5.2	.58	2.66	.66	5.5	.75	7.0	2.0	5.728
II.	4.50	1.00	5.2	.84	3.00	.33	4.2	.62	6.0	1.5	5.274
I.	2.83	.55	2.83	.94	1.83	.27	3.25	.37	4.25	.87	3.709
II.	2.66	.44	3.83	.66	1.50	.50	3.00	0	3.25	.75	3.264

The results of all three forms of the experiment indicate that the complex series has the advantage in each case. Not only is this true for the first acquisition, but it holds as well for the second learning. We should have, therefore, to modify somewhat an opinion which is often expressed, namely, that the more difficult a thing is to acquire in the first place the more readily it is regained at a later time. There must, of course, be an early limit to the arbitrary complications which can be introduced in the above way; for as soon as we take a content which cannot be immediately apprehended as a whole, we shall have a division of attention. 'Greater complexity' in a content does not mean a greater number of different objects in juxtaposition; it means greater variety in unity. A complex content is one which, while it represents many possibilities of distraction, *i. e.*, stands for much past experience, may yet be immediately taken in as a whole. Each member of a series, then, must have unity as well as variety, and when we reflect that the combinations used in the tests — a syllable, a color and a point in space — were purely arbitrary and perfectly new to the subjects, it seems somewhat remarkable that there should have been any uniformity at all in the results. The general conclusion to be derived from the three experiments is this: *that the most complex series of syllables (the one with space and color) was learned more easily than the simplest series; and that both elements in the complication (as proved in part 1 of space, and in part 2 of color) contributed to this result.*

## II.

## ATTENTION AS DETERMINED BY THE COMPLEXITY OF THE PRESENTED CONTENT.

In order to examine the assumption that attention or interest is centered in a complex as opposed to a simple object, several tests were arranged in which a complex and a simple content bid against each other for attention. Experiments relevant to this situation have been made by Mrs. Hill.<sup>1</sup> Her method was this: two cards which first had like figures on them were laid before the subject and he was told to select one by taking it in his hand. Slight differences were then made in the two cards, *e. g.*, a small cross (x) was put in the corner of one card and again the subject made a choice. Her conclusion was that whatever attracted attention was most likely to determine choice in its favor. The general tendency was for attention to go to the card which had had the mark or color added to its other content. Münsterberg<sup>2</sup> has performed experiments to compare the judgment of time-intervals as dependent upon the 'filling' of those intervals. He found that in the case of two equal time-intervals that one was judged shorter which was filled with the more complex content.

The statement is frequently made that, when two fields compete for attention, the intricate or interesting one is preferred. The object of the following tests was to demonstrate as much in the instance of simple geometrical figures. The question as to which of two fields makes the stronger appeal to interest may be divided into two inquiries, namely, which attracts attention first, and which holds attention longer when it is attracted. The distinction is of course a purely practical one. The first point—the attraction of attention—was tested in this way. The subject was asked to fixate a spot marked midway between two simple designs and then to turn the eyes to either side he chose. In order that the first glance might be a real choice he was told that he might look at either picture but at *only one*. The cards were at such a distance apart that a vague impression of the figures could be got in peripheral vision when the

<sup>1</sup>*Am. Jour.*, Vol. 9, p. 587 ff.

<sup>2</sup>*PSY. REV.*, Vol. 1, pp. 51-56.

eyes were centered on the point between. A few tests were given to each subject in which the two figures were just alike, and in no case did there appear to be any noticeable preference for looking to the right or to the left. Tests were then given in which ten pairs of pictures were shown, each pair consisting of one simple and one somewhat more complicated figure, as in Fig. 3.

The more intricate designs appeared sometimes on the right and sometimes on the left, but not in any regular alternation or

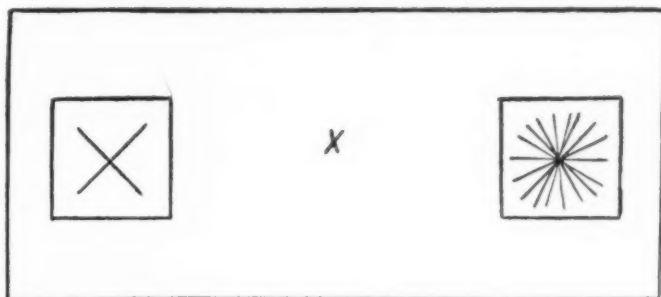


FIG. 3.

rhythm. The question then stood: out of these *ten* possible choices how many times would the subject be attracted by the complex figure?

Out of ten persons, nine chose the complex the great majority of times. Thus, *A* chose the complex nine times out of ten, or 90 per cent. of the time.

<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>	<i>F.</i>	<i>G.</i>	<i>H.</i>	<i>I.</i>	<i>J.</i>
90%	100%	60%	90%	90%	90%	10%	100%	100%	100%

The introspective evidence was expressed in terms of 'interest' and 'curiosity.' The subjects were sometimes able during the few seconds of fixation on the mid-point to take in the simpler figure, and the tendency then was to look at the doubtful or indefinite one. The attraction was the indistinctness or the problematic character of the picture. One subject showed a marked exception to the others: *G* almost invariably looked at the simpler figure. His explanation was that the simpler one made a sharper contrast with the background.

## 2.

The second point — the holding of attention — was measured in this way. The subject was told to let the eyes oscillate between the two fields, lingering as long as he cared to on either side. These eye-movements were watched and recorded by the operator on a smoked drum, the time being kept by a seconds marker. There is a chance for considerable error in this method, inasmuch as the oscillations are often very rapid and some time is taken up by the operator in catching the eye-movements and responding to them upon the electric key which was used to make the records on the drum. The results represent the percentage of cases in which the complex object held attention for the greater part of a given time. Thus, *A* looked at ten pairs of pictures, and each time she looked longer at the complex one of the pair than at the simple one.

	Complex.	Middle Case.	Simple.	
<i>A</i>	100%			Only nine tests made with <i>C</i> and <i>D</i> instead of ten.
<i>B</i>	60	30%	10%	
<i>C</i>	60	30		
<i>D</i>	70	10	10	
<i>E</i>	100			

The left-hand column gives the percentage of cases (out of ten) in which the complex figure held attention longer; the middle column shows the percentage of cases in which the time was evenly divided between the simple and complex, and the third shows the times when the simpler design was dwelt on longer.

## 3.

A different plan was also used for trying whether attention would be held any longer by a more intricate geometrical figure. Drawings were taken such as are frequently employed to illustrate the fluctuations of attention, *e. g.*, the flight of steps. The subject was made to watch first the simple outlined book-cover *a*, Fig. 4, and to record, by pressing a rubber bulb, the changes in attention. He was next asked to look at a more complex diagram *b*, Fig. 4, and to register in the same way the fluctuations which these blocks sustained. The records were traced on a revolving drum, the pressure on the bulb sending

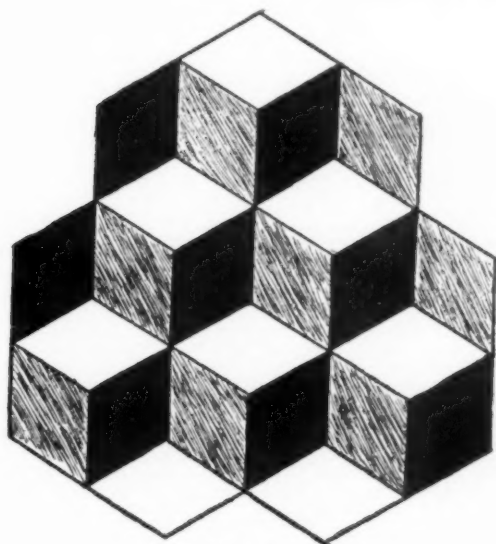


FIG. 4a.

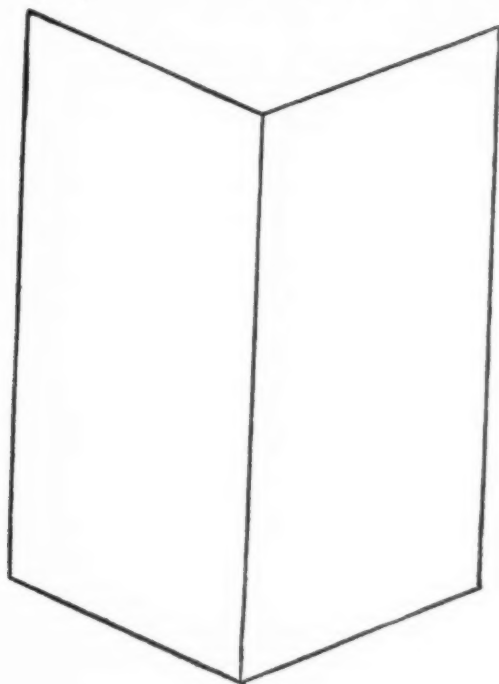


FIG. 4b.

the pointer up, and the rate of rotation was kept by a seconds marker so that the results had this form (Fig. 5).

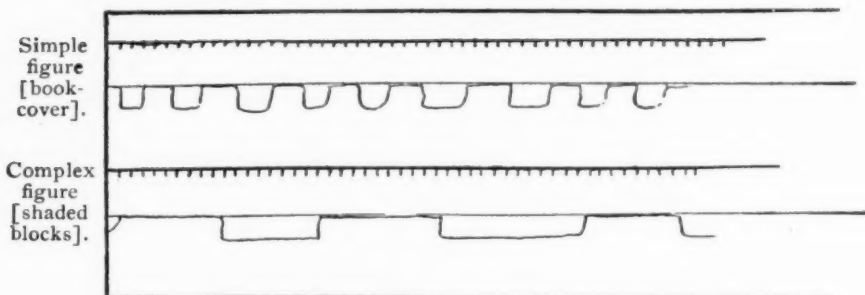


FIG. 5.

The numbers below stand for the number of fluctuations occurring within a given time, *e. g.*, to 100 changes with the simple figure there were only 50 with the complex, which means that the complex one held attention for twice as long as the simpler one did.

	A.	B.	C.	D.	E.	
Simple book cover.	100	100	100	100	100	Fluctuations.
Complex shaded blocks.	50	70	35	35	20	"

The actual number of seconds during which the pictures remained constant, was approximately 4, 5 or 6 for the complex, and 2 or 3 for the simple.

To restate the general point of view which the above experiments in memory and attention serve to illustrate, we may consider the relation of meaning as a principle of association to the two principles commonly advanced, namely contiguity and interest. A content has complexity or meaning according to the amount of past experience it represents, or, in proportion to the appeal it makes to habits of mind already fixed. Simple contiguity is the initial type of meaning — its lowest form — a mere chance mechanical juxtaposition: the connection between two things which have an intrinsic interest in each other, or 'necessary' bond, represents the highest or last form of meaning. These two conditions of memory are indispensable to one another. Two contiguous things would never be perceived as



contiguous unless we *attended* to them together, had some slight degree of *interest* in them as together. On the other hand any two things which are intrinsically germane are by that very fact contiguous in consciousness. It seems, therefore, not inappropriate to call contiguity and interest the two extremes of the process by which significance is attained,<sup>1</sup> and to suppose that many repetitions of a thing in our progressive experience may transform the 'merely' contiguous into the 'conatively' or intrinsically contiguous. To a living organism repetition is never mere iteration; but the effects of successive presentations are cumulative. There is added, for example, the emotional element of familiarity. Or, beginning at the other end, we see that discrimination and association go inevitably together (two things held in mind as different are by that act associated), and hence anything which has many differentiae — much meaning — has many associations, *i. e.*, has been present many times in past experience. The greater its intrinsic appeal, the more must an object have been *worked over* in experience.

On the structural side, then, meaning is contiguity often repeated; on the functional side, it is attention or interest. Repetition is the means to the acquirement of significance, and the fact of internal interest in a content stands for the significance acquired in the past. The two factors are complementary; for with richness of content fewer repetitions are needed to make a lasting impression, whereas a poor material must be repeated many times.

### III.

#### AN INVESTIGATION OF THE PROCESS OF JUDGMENT AS INVOLVED IN ESTIMATING DISTANCES.

BY M. L. ASHLEY.

### I.

#### NATURE OF THE EXPERIMENT.

It is a well-known psychological fact that our judgments are often due to habits or influences of which we are not at the

<sup>1</sup>Of interest in this connection are a few paragraphs by Stout on the 'acquirement of meaning,' 'Manual of Psy.,' Bk. I., Ch. II.

time fully aware. It is less generally recognized that we may mistake the factor upon which the judgment is really based, and least of all is it commonly supposed that our judgments may rest upon elements which we imagine ourselves to be consciously and definitely neglecting. That such is nevertheless the case is shown by the experiment we are about to describe.

The object of the experiment was twofold. In the first place, it was thought that, if by some means the visual and the tactual-motor factors, upon which we mainly rely in our estimates of depth, could be brought into conflict, the resulting judgments might serve to indicate the respective rôles of these factors in our spatial judgments formed under normal conditions.<sup>1</sup> In the second place, since the judgment would be formed under conflicting tendencies, one might reasonably expect to obtain certain data which would be of value in bringing to light in some degree the psychology of the judging process itself.<sup>2</sup>

In order that these purposes of the investigation might be accomplished, it was essential that the observer should be ignorant of the real situation and should suppose that an object which he reached for and touched was at the same distance from him as an object which he saw. In fact, the problem was to find what effect would be produced on one's judgments by a discrepancy, either recognized or unrecognized, between the visual and tactual-motor distances of objects whose distances were supposed to be the same. Indeed, in most of the experiments the object touched and the object seen were supposed to

<sup>1</sup> Somewhat similar investigations have been made to determine the comparative accuracy of touch and vision, as, *e.g.*, 'A Comparison of Sight and Touch,' by Bowditch and Southard, *Journal of Physiology*, III., pp. 232-245. 'The Perception of Space by Disparate Senses,' Jastrow, *Mind*, XI., p. 539 f. But these experiments were concerned with spatial estimates of the visual and tactual-motor factors when employed separately. Another related experiment is that of Stratton on 'Harmony of Touch and Sight,' *Mind* (N. S.), VIII., p. 492. In this case we have mainly, as in a number of similar investigations, the formation of new habits and coördinations, rather than the direct measuring of one factor against the other.

<sup>2</sup> It may be said of such experiments as K. Marbe's 'Experimentell-psychologische Untersuchungen über das Urtheil,' that they deal with cases of comparatively simple and direct reaction, in which conflicts are not clearly brought out.

be one and the same. To meet these requirements the following apparatus was constructed :

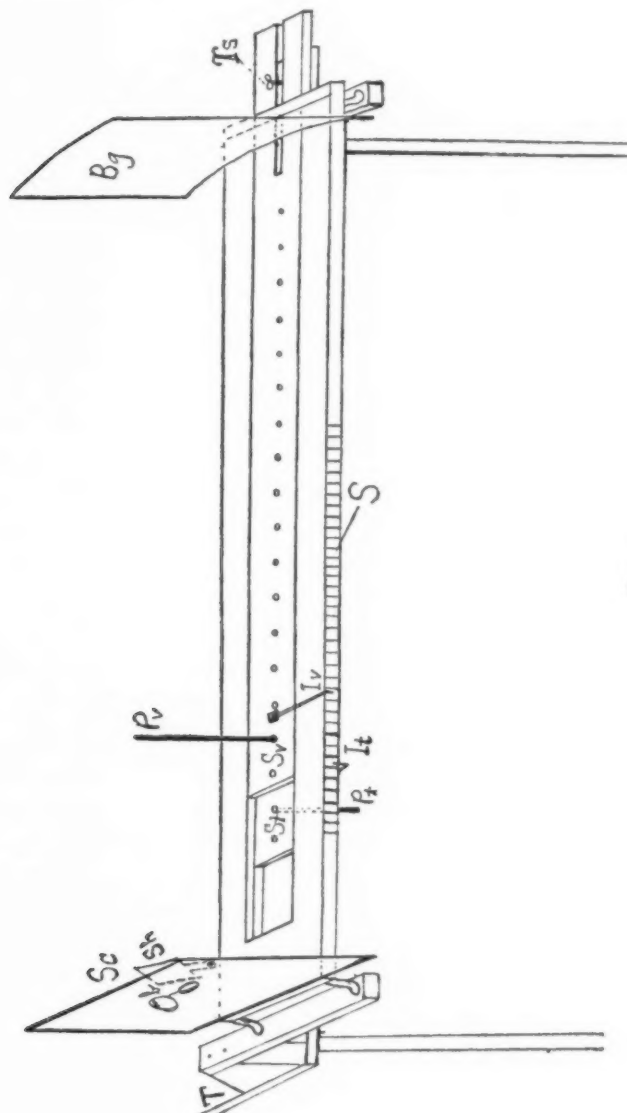


FIG. 1.

Two narrow strips of hard wood were fitted one above the other in a groove in such a manner that they could slide back

and forth either together or independently of each other. Small holes, two centimeters apart, were bored through the two strips, through which a long pin could be pushed, so that it could be seen from above and touched from below. Or, two pins could be used, one of which, *Pv*, would be carried by the upper slide *Sv*, and the other, *Pl*, by the lower slide *Sl*. *Ts* is a thumb-screw which passes through a slit in *Sv* and clamps *Sv* and *Sl* together when desirable. The framework with the slides in place is mounted on a table, *T*, in such a manner as to allow freedom for the arm in reaching. At the front is a screen *Sc*, with a narrow opening *Ob*, through which the observer looks with both eyes at *Pv*. *Ob* is provided with a shutter *Sh*. The upper slide is furnished with an index *Iv*, and the lower slide with an index *Il*. These indices indicate on the millimeter scale *S* the distance either pin has moved. *Bg* is a black cloth background. The arrangement of the apparatus in the diagram (Fig. 1) shows the lower pin the nearer of the two to the observer.

## II.

### EXPERIMENTATION AND RESULTS.

In order to determine the degree in which one's spatial judgments could be influenced by the abnormal conditions employed, it was first necessary to find within what limits the movements of the pins could be detected under normal conditions. Since each pin could be moved separately, or both could be moved together, either forward or backward, six difference thresholds would be required for each observer for the distance which was to be employed as a reference point, or standard, from which the pins were to be moved. As a convenient standard distance a point on the scale was chosen and maintained throughout the experiments, which brought the visual pin forty-two centimeters from the eye of the observer.

To obtain the threshold for vision alone the pin *Pv* was exposed a sufficient length of time for the observer to get a clear impression of its location; then the shutter was closed, the pin moved and the shutter opened, about three seconds having been required to change the position of the pin. The observer then judged 'nearer,' 'further,' 'same,' or 'uncertain.' These

judgments are indicated in the tables by *n*, *f*, *s*, and *x* respectively. To find the threshold for the tactual-motor side alone, the shutter was kept closed throughout. After reaching and touching the standard, the observer lowered his arm while the pin *P* was being moved. He then raised his arm and after reaching and touching the pin, judged as in the case of vision alone. Practically the same time interval was required for moving the pin as in case of vision. Where vision and touch-movement were combined, the observer saw and touched the pin at the same time. The shutter was then closed and he lowered his arm. The pin having been moved, he again raised his arm and the shutter was opened when he was about to touch the pin, in order that he might receive the tactual-motor and the visual impressions as nearly as possible at the same instant. The time occupied for the shifting of the position of the pin and the form of the judgments were the same as before.

The thresholds were determined mainly by the method of right and wrong cases, the amount of change which was correctly judged in seventy-five per cent. of the cases being taken as the threshold. The unit of change was one half of a centimeter for vision alone or for vision and touch combined; for the tactual-motor form alone the unit employed was one centimeter. During the determination of the thresholds as well as in the subsequent experiments, the observer sat at such a height that his line of vision fell upon the black background and well above the slides which carried the pins. This precaution was taken to prevent his receiving any assistance through comparison with other objects.

When two pins are employed, as in this experiment, there are ten ways in which they can be placed at unequal distances from the observer, who supposes he sees and touches the same pin, while in reality he touches one and sees another. (Only in the exposures employed as standards were the two pins actually at the same distance from him.) These ten positions for the pins are shown below. Suppose the observer to be located at the left of the pins. Where a pin has been moved, its position in the standard will be indicated by a dotted line. Arrows show the direction of movement from the standards which are

figured directly above in each case. These varieties are arranged and numbered in the order in which they were meant to be given, though it was not found convenient to maintain this order strictly throughout the experiment.

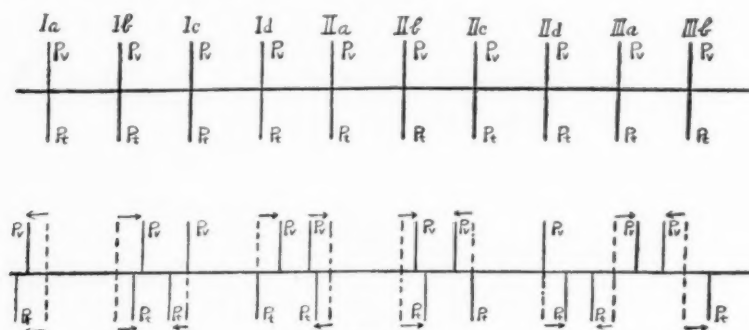


FIG. 2.

It will be noticed that in *Ia*, *Ib*, *Ic* and *Id*, *Pt* is always nearer to the observer than *Pv*, while in *IIa*, *IIb*, *IIc* and *IId*, *Pv* is nearer than *Pt*. In *IIIa* and *IIIb*, *Pv* and *Pt* have moved in opposite directions and their positions in *IIIa* are the reverse of those in *IIIb*.

Since the main object of the experiment was to study the conflicts brought about between the spatial impressions received from *Pv* and *Pt*, the combinations *Ia*, *Ib*, *IIa* and *IIb* were not given so much attention as the others and were omitted after a few trials. Accordingly, it will suffice for our purpose to show in tabulated form the results obtained under *Ic*, *Id*, *IIc*, *IId*, *IIIa* and *IIIb*. These results appear in the following tables, which are numbered in accordance with the previous description. The errors in the judgments will be more apparent if the threshold tests are compared with them, and readily to permit this a table of thresholds will be given first.

## THRESHOLDS.

	A.	B.	C.	D.	E.
<i>Pvn</i>	1.5	2	1	1	.5
<i>Pvf</i>	2.5	4.5	1.5	3.5	1.5
<i>Ptn</i>	3	3	4	2	2
<i>Ptf</i>	2	2	3	2	2
<i>P(v)n</i>	1.5	2	2	1	1
<i>P(v)f</i>	1.5	3	1.5	2.5	1



*Pvn*, *Pvf*, *Ptn*, and *Plf* signify that *Pv* or *Pl* has been moved nearer or farther away. *P(tv)n* and *P(tv)f* mean that hand and eye are combined and that the movement is nearer

Ic							Id						
<i>Ptn.</i>	<i>Pvs.</i>	<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>	<i>Pts.</i>	<i>Pvf.</i>	<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>
1	o	x	n	n	s	s	o	.5	n	f	n	s	f
2	o	x	n	n	n	n	o	1	f	f	n	s	s
3	o	x	s	n	n	n	o	1.5	f	f	s	f	f
4	o	x	n	n	n	s	o	2	x	f	n	s	f
5	o	x	n	n	n	f	o	2.5	f	f	f	x	f
6	o	x	n	n	n	n	o	3	f	f	f	n	f
7	o	x	n	n	n	n	o	3.5	f	f	f	s	f
8	o		n	n	n	n	o	4	f	s	f	f	t
9	o				n	n	o	4.5	f	f	f	f	f
10	o		n		n	n	o	5	f	f	f	f	f
11	o				n	n	o	5.5	f	f	f	f	f
							o	6	f	s	f	f	f
							o	6.5		f	f	f	f
										f	f	f	f
											f	f	f
												f	f
													f
													f

and further respectively. Throughout the tables the vertical columns headed *A*, *B*, *C*, *D*, and *E*, contain the results obtained from the respective observers. In the table for thresholds

IIc.							IID.						
<i>Pts.</i>	<i>Pvn.</i>	<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>	<i>Plf.</i>	<i>Pvs.</i>	<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>
o	.5	x	s	n	n	s	1	o	x	s	n	s	n
o	1	n	s	n	s	n	2	o	x	s	f	f	f
o	1.5	n	s	n	s	n	3	o	x	f	f	s	f
o	2	n	s	n	x	n	4	o	x	n	f	f	f
o	2.5	n	s	n	n	n	5	o	x	f	f	f	f
o	3	n	s	n	n	n	6	o	x	s	f	f	f
o	3.5	n	n	n	n	n	7	o	x	f	f	s	f
o	4	n	s	n	n	n	8	o	x	f		f	f
o	4.5	n	n	n	n	n	9	o	x	f		f	f
o	5	n	n	n	n	n	10	o	x			f	f
o	5.5	n	n	n	n	n	11	o	f				
o	6	n	n	n	n	n	12	o	x				
o	6.5		n	n	x	n	13	o	x				
o	7		f	n	n	n	14	o	x				
o	7.5		n		n	n	15	o	x				
o	8		n		n	n							
o	8.5		n										

the extent of the required movement for each observer is given in centimeters. The letters *Pvn*, *Pvf*, etc., indicate the kind of change for which the threshold was obtained. In the tables

IIIA.							IIIB.						
<i>Ptn.</i>	<i>Pvf.</i>	<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>	<i>Ptf.</i>	<i>Pnv.</i>	<i>A.</i>	<i>B.</i>	<i>C.</i>	<i>D.</i>	<i>E.</i>
.5	.5	x	s	n	f	n	.5	.5	n	s	s	n	n
1	1	x	s	x	f	f	1	1	n	s	n	n	n
1.5	1.5	f	s	f	x	f	1.5	1.5	n	s	n	n	n
2	2	x	s	f	f	s	2	2	n	s	n	s	n
2.5	2.5	f	n	f	s	f	2.5	2.5	n	s	f	n	n
3	3	f	s	f	f	f	3	3	n	n	f	x	n
3.5	3.5	f	n	x	n	n	3.5	3.5	n	n	f	s	n
4	4	f	n	f	f	f	4	4	n	f	f	x	n
4.5	4.5	f	n	n	f	f	4.5	4.5	x	s	f	f	s
5	5	f		f	n	n	5	5		s		f	n
5.5	5.5	f		n	f	f	5.5	5.5				f	n
6	6			n	f	f	6	6				f	n
6.5	6.5			s	f	f	6.5	6.5				f	n
7	7			n	f	f	7	7				f	f
7.5	7.5			n	f	f	7.5	7.5				f	n
8	8						8	8					
8.5	8.5						8.5	8.5					

*Ic, Id, IIc, IId, IIIa* and *IIIb* the first two vertical columns headed *Pvn*, *Pvf*, etc., contain the extent in centimeters of that particular form of change which the observer judged 'nearer,' 'further,' etc., as the case might be. *Pvs* and *Pts* indicate that *Pv* and *Pt* were stationary.

We see in the table for thresholds that the lowest thresholds were obtained for *Pvn*, while for *Pvf* a much greater change was required.<sup>1</sup> The reverse was true in case of *Ptn* and *Ptf*. The thresholds where eye and hand were used together fall between those in which either eye or hand was used alone. It would appear that in this last case the poorer criterion was not discarded in favor of the better, but entered as a hindrance, and the result was really the combined effect of the two. It should be remarked in this connection that where the change was great, the eye and hand seemed to reinforce each other when the movement for each was in the same direction, and that in such cases there was a marked increase in the observer's assurance.

It was not possible to obtain the same number of judgments of each kind from all of the observers, because the nature of the experiment was such that, if at any time the observer had sus-

<sup>1</sup> This result is in general agreement with those obtained by Wundt, Arrer and others, though the thresholds are rather higher. M. Arrer, 'Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung. *Philos. Studien*, XIII., 116-161, 222-304.

pected that the pin he saw was not the same one he touched, his subsequent judgments would have been valueless. Accordingly, when the extent of movement had been sufficiently increased for marked perplexity to be manifest, it became necessary to change to some other form of the experiment. It would hardly be expected that all of the observers could be brought to the same point before recognizing the discrepancy between the two pins.

The tables show that the several observers, despite certain irregularities, were influenced in very different degrees by the visual and the tactual-motor factors. Thus, in table *Ic* (*Pt* nearer and *Pv* stationary) it appears that *A*, who is of the visual type, was in doubt when *Pt* alone had changed its position, while all of the other observers judged more in accordance with the movement of *Pt*. Again, in table *Id* (*Pt* stationary and *Pv* further), *A* judged 'further' quite regularly. In tables *Iic* (*Pt* stationary and *Pv* nearer) and *IId* (*Pt* further and *Pv* stationary), *A*'s preference for the visual factor is clearly shown. In tables *IIIa* (*Pt* nearer and *Pv* further) and *IIIb* (*Pt* further and *Pv* nearer), *A* appears to have followed mainly the pin *Pv*. In tables *Ic*, *Id*, *Iic* and *IId*, *B*, *C*, *D* and *E* are shown to have judged for the most part according to the pin which really moved, whether it was *Pv* or *Pt*, though in tables *IIIa* and *IIIb*, *E* seems to have been influenced more by *Pv* than by *Pt*.

An examination of the tables shows further the results of conflicts which were brought about between the visual and the tactual-motor factors. The subjective effects of such conflicts were various. Sometimes the observer seemed to feel no difficulty and to be quite unaware that his grounds of judgment really conflicted. Sometimes he would experience considerable perplexity, but would be at a loss to account for it. In other instances he would assert that he judged by eye or hand almost exclusively, or, again, that he combined the two, while his results indicated clearly that in reality he did not recognize the bases on which his judgments rested.

To make this clearer we may refer to specific instances. In the case of *A*'s judgments in table *Ic* (*Ptn* and *Pvs*) the effect of the tactual-motor factor is evident, though throughout the whole experiment *A* supposed her judgments to be almost

wholly controlled by vision. At no time was the difficulty understood, though the perplexity and disappointment at not being able to tell whether the pin had moved or not was clearly shown in the tone of voice in which the judgments were given. It will be seen that in every instance reported in this table *A* was unable to detect any change in the position of the pins, though in some instances the pin *Pt* had actually moved more than twice the distance which could be detected in the threshold determinations in which *Pt* alone or both *Pt* and *Pv* approached (*Ptn* and *P(tv)n*). The same holds true in a still more marked degree in the experiment reported in table *IIId*. Here, though previously able to detect a change of two or three centimeters, an increase of fifteen centimeters in the distance of *Pt* did not lead *A* to judge it further, when *Pv* was stationary. In tables *IIIa* (*Ptn* and *Pvf*) and *IIIb* (*Ptf* and *Pvn*) *A* mainly followed the visual factor, but the results of conflict appear in a few instances, particularly in the last judgment in table *IIIb*.

Though often erratic, *B* judged mainly by the hand. Throughout the tables his judgments are more irregular than *A*'s. *B* was inclined to ignore the eye in cases of contradiction and realized, when the discrepancy was considerable, that the visual and the tactual-motor factors did not agree. Tables *IIIa* (*Ptn* and *Pvf*) and *IIIb* (*Ptf* and *Pvn*) give clear cases of conflict between the two elements, in which *B* seemed to follow now one and now the other.

Observer *C* said that his judgments were mainly of an immediate muscular sort wherever the tactual-motor factor was available. He could judge very well by the eye, he thought, but preferred the hand. As his judgments were given with great care, his results and comments are the more significant. Some of his last judgments given in table *Id* (*Pts* and *Pvf*) were ventured with considerable hesitation. Near the end of the series in table *IIc* (*Pts* and *Pvn*) he said that he was laying a good deal of stress on the hand, while, as the table shows, *Pt* was really stationary throughout. Thus, when *Pv* had approached as far as seven centimeters, he seemed to think it was *Pt* which had given him the main evidence of approach. After the last judgment in table *IIIa*, which was given as 'nearer'

(*Ptn* 4.5 cm. and *Pvf* 4.5 cm.), he said he was combining the two elements, and while he thought *vision* the more important factor, he did not disregard the hand. According to his threshold for *Ptn*, he would have confidently judged *Pt* alone to be nearer. At the end of the series of judgments given in table *IIIb* (*Ptf* and *Pvn*), *C* said he judged mainly by touch, and remarked that the tactual object seemed further than the visual one. It appears that at about the middle of the series, where, according to the thresholds, the conflict should have made itself felt, he changed from the visual to the tactual-motor factor as a basis for his judgments, but did not notice the discrepancy between the two pins till near the end. It will be noticed that at the end the pins were actually nine centimeters apart.

Observer *D* placed most reliance on the hand. She remarked once early in the series of table *Ic* (*Ptn* and *Pvs*) that the tactual object seemed nearer than the visual one, but she appeared to disregard this occurrence afterwards. Near the end of table *Id* (*Pts* and *Pvf*), *D* remarked, after having given a judgment 'further' with some hesitation, that the hesitation arose from using hand and eye together, since the visual estimate came more slowly; then, in reply to a question, *D* said that there was *no disagreement* and that *the same result would have been obtained from either alone*. These introspective observations were the more astonishing because *D* had but little faith in the eye compared with the hand as a means of estimating distance. In spite of the fact that *Pt* remained stationary, *D* said the visual estimate of 'further' came more slowly, that there was no disagreement, and that either alone would have given the same result. We have here a clear case of error as to the source from which the judgment was really obtained, and the ascribing to tactual-motor elements of an impression really originating in visual processes.

In table *IIC* (*Pts* and *Pvn*), in the case of the second of the judgments  $\alpha$ , *D* said that the object as compared with the standard looked the same and felt further. *D* remarked, in addition, that both eye and hand were employed, and that it was not so hard to use both as it had been before. We see, therefore, the same sort of error that was noticed in table *Id* (*Pts* and *Pvf*).

In connection with the results given in *IId* (*Ptf* and *Pvs*), *D* said that in case of any apparent difference at first, the visual object was always made to agree with the tactual, but that the *same final result would have been attained with either alone*. In table *IIIa* (*Ptn* and *Pvf*), *D*'s judgments are very irregular, and at the end vision and touch could not be made to agree. The same disagreement was experienced near the end of the series in table *IIIb* (*Ptf* and *Pvn*), though in table *IIIb* the tactual-motor side appears the more influential of the two.

In *E* we have a pronounced visual type. In *Ic* (*Ptn* and *Pvs*), *Id* (*Pts* and *Pvf*), *IId* (*Pts* and *Pvn*), and *IId* (*Ptf* and *Pvs*) the hand did not appear to play much part and no disagreement was noticed. In table *IIIa* (*Ptn* and *Pvf*), however, this manner of judging seemed to be reversed in a few instances. At the end of another series like *IIIa* and of equal length (not appearing in the tables), in which the first judgment was 'nearer' and all the rest 'further,' *E* said that where the change was great, he tended to judge by means of the hand and had lately been doing so. It is seen, however, that while the pin was being judged further, *Pt* was constantly coming *nearer*. In the last judgment but one in table *IIIb* (*Ptf* and *Pvn*), in which *E* judged 'further,' he noticed the disagreement between the two objects.

In addition to the results which appear in the tables, a few others were obtained from the same observers, which did not differ materially from those tabulated. Other observers also took part in certain phases of the experiment with like general result. In one instance the observer, although he knew the nature of the apparatus, was misled into confidently judging the visual pin nearer, when only the tactual-motor one had approached.

While the experimentation was rather too limited in respect to both the number of observers and the number of judgments obtained to justify any sweeping generalizations, the results are not without significance in regard to certain aspects of our spatial judgments, and are at least suggestive as to the judging process itself. It appears that with persons of either pronounced visual or motor type the corresponding psychical factor tends in judg-



ments of this kind to be emphasized to the neglect of the other in case they are combined. Whether ultimately the visual factor depends on the tactual-motor factor for its real significance, and whether, if cases of marked discrepancy between them were long continued, the visual would yield to the tactual-motor, remains a question. Such a view certainly has much in its favor, and it is not improbable that forms of experimentation could be devised which would go far towards deciding the matter.

The outcome of the experiment certainly adds evidence in support of the view that the bases of our judgments may be in a great measure subconscious, in the sense that they are not explicitly recognized. The introspective testimony given by the observers indicates that, as was mentioned at the beginning, the judgment may be the product of tendencies or habits which we not only do not ordinarily recognize, but which we can not correctly evaluate when conflict brings them to our notice. The most striking of the experiments show that we may be predominantly influenced by factors of which we are clearly conscious and which we definitely suppose ourselves to be disregarding. In fact, the judgment may apparently be viewed as the product of more or less conflicting habits, in which the form it finally takes is a sort of resultant of various forces; and the sharper the conflict among these forces, the greater may be one's sense of uncertainty and distrust as to his correctness.

I am under obligations to those who kindly gave their services as subjects in the experiment. For them the work must have been somewhat uninteresting, inasmuch as they were necessarily kept in ignorance of the real conditions under which they judged and of the real purpose of the investigation. I am indebted to Professor J. R. Angell, not only for various helpful suggestions, but also for considerable assistance in constructing and testing the apparatus.

## DISCUSSION AND REPORTS.

### AFTER IMAGES AND ALLIED PHENOMENA.

I have a frequent experience in connection with after images which it may be worth while to record, especially as others may be able to repeat or confirm my observations. What I have to remark is not the result of deliberate experiment at first, but is a spontaneous occurrence and what follows it is the consequence of experiment.

I have in my life often experimented with after images, and perhaps this fact makes me susceptible to them and to the observation of them when they occur without the effort to produce them. However this may be, I often notice an after image of a bright object in the field of vision when I am not trying to produce it. It of course arrests my attention and I immediately turn to observe it. As usual it quickly fades. I then try to reproduce the after image by experiment and as generally fail as I try. No amount of effort will reproduce it as before. I may obtain a faint one, but usually can obtain none at all. But the interesting phenomenon in connection with the spontaneous after image that arrests my attention is the fact that I have uniformly observed that it occurs only when I am in a state of abstraction. Thus if I am looking at a lamp or bright ring and at the same time not thinking of the object on which vision is actually fixed, the after image is almost certain to occur with great distinctness if I happen to turn the head to one side and the background is favorable. If I try to repeat the after image by looking purposely at the light, I utterly fail. The production of it seems to be related in some way to the connection between fixation and inattention. It may be worth studying in this connection the influence of attention upon the action of chemical forces in the retina. Of course something of this kind may already have been done, but if so it has not been my fortune to see it, as my studies have not enabled me to keep abreast with the scientific and physiological side of this matter. But the phenomenon which I have just described certainly suggests a possible relation between attention and the amount of chemical action in the retina.

There is another phenomenon which is possibly connected with related functions. When mentally preoccupied and having the eyes fixated on a given point or object I often notice the disappearance of a

part of the indirect field of vision. I have tried to see whether it might not be due to the falling of the object on the blind spot, but uniformly discover that it is not, as the disappearing object may be on the side of the retina opposite the blind spot. On careful experiment and observation I find that the disappearance is directly related to the degree of abstraction, and that I can reproduce it artificially, if I am successful, as I sometimes am, in effecting the abstraction necessary and at the same time the proper adjustment of attention. It is difficult to produce the artificial abstraction required, but when I am successful I effect the disappearance of the object, which immediately reappears the moment attention is given to it without altering the fixation of the eyes. The effect seems to be that of making clear an actual impression, while attention in the previous experiment seems to destroy an after image. Why is this the case? I of course have no answer to this question. It is simply an interesting phenomenon to find the fact, which is apparently the converse of the first experience described. In the former, concentration of attention is conducive to the appearance of after images, and in the latter this concentration tends to extinguish real impressions. The latter may be a normal *retrécissement du champ visuel*, but why the former should not also illustrate the same fact is the phenomenon of interest. JAMES H. HYSLOP.

#### THE EARLY COLOR SENSE—FURTHER EXPERIMENTS.

In a discussion of the results arrived at in my article on the early color sense, *PSYCHOLOGICAL REVIEW*, Vol. X., No. 1, p. 37, Dr. Edridge-Green has said that the results are entirely due to the illuminosity of the experimental color cards and not to the colors themselves. His theory is that children are color-blind during the first years of life and that a child sees 'all objects as they are seen in a photograph, that is, in different degrees of black and white.' Also in Professor Chamberlain's comprehensive work entitled 'The Child,' in the Contemporary Science series, on page 79, we find that Garbini divides the development of the child according to the evolution of the color sense into six periods: "The fourth period being the 'Chromatic' period—from the sixteenth to the twenty-fourth month of life. The child continues to have more and more delicate photoæsthetic and visive perceptions and begins to have first chromatic perceptions—red and green." As all the experiments recorded in my article were concluded before the child was twelve months old, according to this theory also the results were due to some stimulus other than that of color.

In a discussion which followed a paper I read on this subject before the London branch of the British Child-Study Association, Dr. Edridge-Green said that children could not perceive colors till they could name them successfully and correctly. Allowing for an instant that my experiments give a result due entirely to illuminosity, they then show a wonderful power of remembering and discriminating between the various and often only slightly varying degrees of illuminosity. If the child's power of associating words heard with things seen is strong enough to enable him to name colors when first perceived, it must also be strong enough to enable him to associate the names of colors with the different degrees of illuminosity to which the results are said to be due. But it is the weakness of this power of associating words heard with things seen which spoils the color-sense experiments of Preyer, etc.

With the idea of finding out if illuminosity does play a very great part in obtaining the results by the methods I used, the following experiments were carried out. I obtained a dozen cards of different grays without gloss and laid them out on a table in a room previously arranged for this experiment. Then I took my yellow, blue and red cards and had the light gradually shut out of the room till one of the three colored cards could no longer be seen as such. Stopping the darkening of the room at this period I picked out the gray corresponding to it under these conditions. More light was shut out by my assistant till another was not visible as such, and a gray again matched with it. After I had got the colors matched in this manner my wife, who was assisting me, went through the matching process exactly as I did, and her results compared exactly with mine.

From the matched colors and grays I prepared test cards by cutting a piece about 1" long by  $\frac{1}{2}$ " wide, of very irregular outline, and pasting it on the corresponding gray. If he were color blind to all colors, as assumed in theories mentioned in first part of this article, he should not be able to pick out this irregular patch of color on the gray. But as soon as I showed him a test card his eyes were immediately fixed on the colored patch, and he made an effort to pick it off. These experiments began on his 492d day (16 months old). On the 498th day I tested him in the same way, except that the colored pieces of paper were now only  $\frac{3}{8}$  of an inch square and stuck in various positions on the gray. Sometimes the color would be in the middle and sometimes in the corner of the gray card.

In each experiment he was immediately successful. No more was done in this way till the 616th day, when we had a new carpet laid in

one of our rooms; this carpet had a pattern of such a nature that large pieces of it were of the same decided color. I cut some small squares from the colored papers nearly  $\frac{1}{2}$ " square, and placed them on the three principal colors while he was out of the room. When he came in I picked up one piece and told him to find some more. He started eagerly to look for them and as soon as he saw one would pick it up with delight. Altogether he picked up 12 pieces, being four different-colored papers on each of 3 different colors in carpet, in this way. This test occurred to me because I noticed that he would find the smallest object whenever it differed in color from the ground on which it rested, and of course sometimes the illuminosity of object and ground must be equal, thereby rendering the object invisible if theories quoted are correct. On the 617th day I carried out more similar tests, with the same certain results. Then I cut some elliptical-shaped colored papers (size,  $4'' \times 2\frac{1}{2}''$ ) and pasted small triangular pieces ( $\frac{3}{8}$  of an inch side) on them. I do not mention the colors used, as without seeing them it is hard to judge of their suitability, but it is open to others to check my results on other children. The difference of colors is not so important in these experiments, if the child sees the applied color each time a test card is offered, as it only requires a large number of combinations to make sure that the illuminosity of the applied color does agree with the illuminosity of the ground color in some cases. And if no failure is reported as long as the colors differ, it is a legitimate conclusion that the results are not due to the illuminosity but to the colors themselves. These last test cards gave the same result, and the child immediately saw the small piece of applied color. On the 618th day I used pieces of wool 1" long, laying them on the carpet while he was absent. As before, I showed him what to find and he only had a difficulty in finding one piece. Though we had laid these pieces of wool down we ourselves had a difficulty in finding the piece that puzzled him for a time, the reason being that the colors were so much alike. He was 21 months old when these last experiments were finished, and I think it only needs some observer to experiment on the earlier months in this way to disprove more thoroughly than I have been able to do, the theories that suppose a child to be color blind.

I would suggest that cards could be prepared by printing small patches of color on different-colored grounds so that the experiments would cover a wide range. We all know how our color sense is ever developing; but should we know our children to be color blind we shall confess with sorrow that we have misunderstood them to a greater ex-

tent than we ever dreamed in our most abject moods. And we should feel that it must indeed be a colorless world for them.

RUFUS E. MARSDEN.

#### MENTAL IMAGERY.<sup>1</sup>

Dr. Slaughter's method was to 'ascertain as nearly as possible the exact behavior of the image during a certain interval of time which after trial was fixed at ten seconds.' Figures drawn on cards were used as stimuli. The subject was allowed to fix his gaze on the figure for an indefinite time. At a signal he closed his eyes. Five seconds later he was told by another signal to watch his imagery carefully and to remember the behavior of it, and, after ten seconds of such introspection, he recorded his results. For stimuli designed to evoke visual imagery various figures were used, such as a black square on a white card, with other geometrical forms, playing cards and printed letters. From these tests and from reports of blindfolded chess players he infers that the inner or imagery visual field is contracted, that only a small portion of what was really seen can be reproduced at any one moment in visual imagery. For the purpose of arousing motor images the subject was (supposedly — there is no definite statement) shown cards with, *e. g.*, a picture of a pendulum or of a circle with a ball on the edge. He was told to get a motor image of this, *i. e.*, to imagine the pendulum swinging and to imagine the ball as rolling around the circle. It is not clear from the article what he asked his subject to do, whether to visualize motion or, in some part of his body, to motilize (so to speak) the specified motion. His subjects here report an actual eye movement in themselves, but this is not either motor or motion imagery. Auditory images were called for by the presentation of a tuning fork, two tuning forks with a beat, slowly dripping water, quickly dripping water, a waterfall, ticking of a watch and whistling of wind. The dermal, gustatory and olfactory images studied were those of plush, clammy hand, hot water, plunge into cold water, the four tastes, and ammonia and alcohol. He states that in all the tests with the exception of the card series (that is presumably the playing cards) drawn figures were used instead of call words (p. 529). It is difficult for the present writer, who considers himself at least moderately skilled in mentally representing his objective experiences with considerable fidelity to the quality of the original sensation, to conceive how, *e. g.*, a clammy hand could be drawn on a card

<sup>1</sup> 'A Preliminary Study of the Behavior of Mental Images,' J. W. Slaughter, *Amer. Jour. Psych.*, October, 1902, pp. 526-549.



without the words 'clammy' or 'this hand is clammy' either printed or called; to say nothing of presenting as an auditory stimulus the line drawing of a waterfall or of two tuning forks with beats! The method used for the study of visual images consisted in presenting something to the actual vision of the subject for purposes of making test conditions and of having them as simple and definite as possible. These were then voluntarily recalled as visual images and their behavior described. The auditory images (as we learn from p. 541) were evoked by call words, but we must object here that if this is the case the study of his subjects' auditory images is not analogous to that of their visual images. The parallel to presenting real sights to the eye is presenting real sounds to the ear; and, in the case of motor and motion images, is the presentation of real motion, *i. e.*, putting his subjects in a swing or on a rocking horse or giving them a good shaking by the shoulders for motion imagery; for motor imagery getting them to do some unusual thing with their arms or legs; and, for olfactory, gustatory, and other images, giving them real sensations belonging to these different sense qualities and asking them on some subsequent occasion if they have observed any mental resuscitation of the originals. I say on some subsequent occasion, because the quality of some of the last-mentioned sensations is, in my own case at least, likely to remain as an after image or memory image (motor, *e. g.*, the rocking of a vessel experienced by one sometimes after disembarking, which has the quality of an after image: objectivity, non-subjective determination, etc.). If this research had been consistently carried out even with call words as stimuli, it would, to be strictly consistent, have thrown out the study of auditory images entirely.

The doubtful value of Dr. Slaughter's experiments is plainly shown in his discussion of his subjects' auditory introspections, *e. g.*, he says that his subject St. 'really has better auditory images than K. and is strongly motor while K. is strongly visual'; and 'when the object stands out complete except for the sound, and the whole situation is arranged so as to point to it, it may seem present as a matter of course whether it actually appears or not, and may seem as clearly distinguishable as any of the other qualities. It is similar to the case of the blind spot in the field of vision.' I think Dr. Slaughter has been misled here by a false analogy. A sense quality is a definite complete and always distinguishable mental phenomenon, and a subject if sufficiently trained in introspection can always say whether he has it or has it not, or feels doubtful about it. It is beyond the province of any one else to tell him that he has it not, when he says he has it.

If therefore the subjects of Dr. Slaughter say they had auditory imagery, he can hardly presume to say that they did not. He says he knows his subject St. had no auditory imagery because St. said he had the other kinds, *i. e.*, the presence of visual imagery is sufficient proof that the other kind did not exist, in spite of the statement of the subject that it did.

To examine in detail some of the verbal reports of the introspection of the subjects, let us take those concerning auditory imagery. After the call word (?) 'tuning fork,' subject St. "Felt tension in tongue but could not really get sound. Had sort of humming located in mouth fairly continuous. No visual image of fork. No kind of outside setting." The only words here that can, by any twist, be made to describe auditory imagery are the continuous humming; but he puts it in his mouth! The only way I can mouth the conception of humming is by means of motor imagery. True auditory humming is imaged in terms of pure sound, and as I wrote these words I mentally heard a boys' glee club humming a part of a song. There is evidently no clear statement on the part of St. that he has any auditory imagery at all, and yet St. is the subject who 'really has much better auditory images than K'! Let us follow St. through several other experiments. In 'Two tuning forks with beats' St. says, "Visual image of two forks. Oscillation of attention between forks that really seemed external and humming in the head. Beats were only rhythm put in by emphasis in humming. Visual image had no setting, seemed just in front in air. It fluctuated considerably." There may have been real auditory imagery here. To 'Slowly dripping water' subject St. says, "No visual imagery. Felt distinct movement in throat." This is real objectified sensation, and not imagery of any kind. "Rhythm intervals about a second long. Word 'drop' was repeated with the rhythm." No description of auditory imagery here! When he says 'drop' was repeated, he fails to tell us how. It might have been auditory; but if so, why did not he say so? It might have been motor, but he does not say that. For 'Quickly dripping water' he says: "Chief part of whole complex was movement in throat." To 'Waterfall' he says: "Visual image of waterfall and water falling over. Movement in throat muscles." And so on. No claim to have had auditory imagery either of ticking of watch or of whistling of wind, unless his statement about the wind that it was 'of whistling around corner of house but no visualization of house' may be taken as a description of auditory imagery. On the other hand, subject K., the inferior aurilizer, disclaims throat

muscle sensations and visualizations in several instances and uses expressions descriptive of real auditory imagery such as 'got sound image,' 'got sound first,' 'sound image very distinct,' 'auditory fairly constant,' 'sound image first,' 'strong wheezy whistle,' etc., though he is constantly describing visual motor and verbal concomitants. Are these concomitants what Dr. Slaughter means by the 'behavior' of the auditory image? In that case, if a pure unaccompanied auditory image existed in his subjects, even for a moment, they would not have any behavior, and so would not come within the scope of his article. It seems, however, that this study of mental images should as stated (p. 526) be 'a study of particular images, if not in their relations, at least *in situ*,' but in the 21 introspections offered us as studies of auditory images, there seem to be only 11 real examples. These, to be sure, are studied *in situ*, or rather all except a few words of the reports are a description of the *situs* and not of the image itself.

Dr. Slaughter remarks on the auditory part of his study that "we are dealing with a vastly more complicated set of conditions than in any of the experiments [visual, motor, motion] previously considered. Images of a visual character are possessed of a certain degree of independence, and the conditions of their maintenance are chiefly, for introspection at least, to be found within themselves. Again, motor images, when taken in isolation, merely require a partial repetition of the original movement or impulse to that movement. But apparently in the case of auditory images, the conditions both of obtaining and holding them have to be brought in from outside. In other words, the study of auditory images is chiefly one of association, both of ideas and sense elements" (p. 543). To these remarks I should like to object: (1) that an auditory image is a psychical phenomena as simple and elemental as is the sensation of a real tone produced by a tuning fork, and (2) that its behavior is not its concomitants. These may or may not influence its maintenance in consciousness, but can not affect its character. Again, let me urge that if motor images, 'when taken in isolation' (where's the behavior gone?), require (for their maintenance, supposedly) a partial repetition of the original movement, then during the time occupied by their partial repetition, the image being comparatively so faint, can not be detected in the mental complex; and that it is tautologous to say that otherwise they require, in order to be maintained, the impulse to that movement, because the impulse differs from the image itself only in the added element of volition.

As to the possibility of subjecting mental images to experimental conditions in the way Dr. Slaughter attempts, I can not but dissent. Had the experiments been carried out for senses other than sight in a manner analogous to that for visual images, he would have had for instance to present a clammy hand to his subjects, let them hold it for some time, wait five seconds, think hard for ten more, and then verbally report their imagery. The results would have been one of three. Subjects would have felt clammy hand, *i. e.*, the imagined touch of it on their own palms or knuckles, as the case may be; or would not have felt it, or they would not have known whether they felt it or not. Any one of these is a direct simple answer, and any psychologist ought to be available as a subject for such research.

I should like to defend my own method as explained in my monograph, and to say that had it been used by him he would probably have found some others of the ten types mentioned by me. What I reported was what I mentally saw, heard, touched, felt, tasted and smelt in various qualities (all imagined, however). Dr. Slaughter says of my observations of my own imagery, 'The faultiness of the method is evident after a direct examination of the images.' I do not know whether he means by this (1) that an examination of the types of images I reported shows that my method of observing them was faulty, that the lack of experimental conditions surrounding the introspections was at fault, or (2) that my method of using these results was faulty (wherein I would with all humility agree with him). If it was the first, I can only say that having frequently caught myself paying close attention to the various imagery concomitant with silent reading, I allowed the reading to lapse, as it were, and entertained the fleeting images uninterruptedly, much as one reading at a window might lay aside his book and look out at a passing military procession. This I would continue for an average of seven and a half minutes, writing down any word that came into my head, that seemed to describe the image then occupying the foreground of my mental content, and ignoring the other images until they came to the fore. I wrote down words also that did not describe images but were part of the imagery. These were classed as verbal imagery (largely auditory).

The records show that, while I made no effort to detain any one image, the average life of any individual one was 8.86 seconds, or very near the time (10 seconds) which he found by trial to be the best. The real issue is whether the method of trying to think in terms of one sense or another according to stimuli presented by an experimenter is better than the method of passively observing the constitution of the

stream of thought. I grant that the attempt to follow the lead of the experimentally administered and therefore controllable stimulus is more scientific in a narrow sense, but I doubt that any results can come from experiments of this nature, because his subject St.'s failure to get the desiderated auditory imagery in nearly 50 per cent. of the instances cited shows only that he did not get it that time and not that he could not mentally image a sound. It was probably scared away by the unnatural environment of the experiment. There is no proof of course that St. is not able to have auditory imagery. In fact he may be, as Dr. Slaughter says he is, a much better *aurilizer* than K. Just here, however, he let K. get ahead of him. It seems anomalous that one should offer as a contribution to the study of auditory imagery the statement that in half the cases there was none. And I should like to protest against this experimental method being substituted for mine with the unqualified remark that mine is faulty. I certainly collected specimens of the species of phenomenon I was studying.

On the whole, Dr. Slaughter's paper seems to me to be an example of not a few that have appeared of late in which the conditions of the experiment are not clearly described; and in the statement of the results the language is not clear, and gives the impression that there are more words than ideas behind them. For instance, in a paragraph on the direction of images he seems to be talking about visual images only, and, if so, it would have been better to say so; and the paragraph is so obscurely worded as to be almost unintelligible. When in the conclusion he says that 'the factors which keep visual images in clear consciousness are their own internal organization combined closely with motor elements,' what can be intended by the 'internal organization' of a visual image other than the image itself; and this is equivalent to saying that the factors that keep a visual image clear are the image and its motor elements; but it is hard to conceive what the motor elements of a visual element of consciousness may be. When he says 'auditory images appear only in connection with an organized associative situation,' what do these three words mean but images of other sense qualities? And if he means to say that auditory images appear only in connection with other images, he says what is not by any means proved; and he helps us not at all, for he does not tell us the nature of that connection. He can not make this statement as a conclusion from his experiments, as he has shown that some of his so-called auditory images were not auditory, but only concomitants, only an 'organized associative situation' mostly motor. When he says that 'images from other sense departments also require such a situa-



tion which is in most cases all that appears,' he seems to me to be guilty of a genuine Irish bull! That there is any doubt of the existence of images of other sense departments than visual, auditory and motor, I cannot accept for a moment. When Dr. Slaughter says that 'the real existence of these images is doubtful,' I understand him to mean their existence in other minds as well as in those of his subjects. He intimates as much when he says that 'our subjects failed to manifest such an elaborate equipment' (as the tactile, gustatory, olfactory, thermal, pain and organic imagery mentioned in my monograph), apparently throwing discredit on the truthfulness of my report of my own images. He says: 'One general question bearing upon the work' is 'whether the images obtained under introspective conditions are the same as the normal images of every-day life.' I can not conceive that the normal images can be studied in any other way save in 'introspective conditions,' and it is only in moments of voluntary or involuntary introspectiveness that we become aware that there are any 'normal working images' at all. So that there is no question as to the sameness of the images. The vital question is whether in subjecting or in trying to subject the flow of images to the control of scientific experiment we may not be changing the nature of these images. I believe that we can not change their sense quality. They remain constant in quality, but the machinery of scientific research may cause them to be obscured or driven below the threshold.

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#### ON LAUGHTER.<sup>1</sup>

A friend of mine once spoke of Sully's writings as sane, eminently sane, but dry. In this 'Essay on Laughter,' however, the reader is fairly deluged and swamped with lively adjectives and figures of speech. 'Blithely' tumbles merrily after 'sprightly' on every page, and 'floods of merriment' chase wildly the 'rillets of joy' of less significant dimensions. Irrelevances in conversation, we are informed, are among 'the recognized tributaries of the river of laughter,' and over all the imp of laughter shrieks his way with uncontrollable joy. Therefore we may say sane, eminently sane, but not dry.

This beautiful largesse of adjectives has, however, also a woful aspect. In the modern world of commerce a business manager would hardly accept a report spread out over 450 pages, a report which might easily have been compressed within 100 pages. I see no reason why the

<sup>1</sup> James Sully, 'An Essay on Laughter, its Forms, its Causes, its Development and its Value,' Longmans, Green & Co., New York, 1902, pp. 441.



psychologic public should not be treated in the same concise, business-like manner as is the head of any ordinary business firm or manufacturing plant. Although somewhat diffuse, excessively sprightly, and never deeply original, still Sully's treatment of the subject is by far the best yet published. It is therefore with diffidence and with deference that a few remarks on the subject are here ventured.

So far as ticklishness is concerned, a very important factor in the production of this feeling is undoubtedly that of the summation of stimuli. In a research of Stirling's carried on under Ludwig's direction<sup>1</sup> it was shown that reflex contractions only occur from repeated shocks to the nerve centers—that is, through summation of successive stimuli.

That this result is also due in some degree to an alternating increase in the sensibility of the various areas in question from altered supply of blood is reasonably certain. The connection of tickling with capillary pulsation is therefore worthy of investigation. As a consequence of this summation-process there could result in many cases and in cases of excessive nervous discharge the opposite of pleasure, namely, pain. This would result from long-continued stimulation or from light stimulation whenever the central nerve cells were possessed of little stability or inhibitory capacity, as in sickness, etc. A number of instances have been recorded of death resulting from tickling and there is no reason to doubt the truth of the statement that Simon de Montfort, during the persecution of the Albigenses, put some of them to death by tickling the soles of their feet with a feather. Mediæval justice and the hidden doings of the Inquisition might reveal many such instances if they were investigated. Lauder Brunton suggests that possibly the different effect of a slight stimulus like the touch of a feather, which causes intense reflex action, and of a gentle but steady pressure of the finger, which gives rise to no reflex action at all, may be due to the stimulation by the latter of two sets of nerves which counteract or inhibit each other.<sup>2</sup> It may be that the effect of steady pressure may cause a general diffused hyperæmia, whereas the stimulation resulting in the phenomena of tickling may and undoubtedly does cause a sudden convulsive hyperæmia which entails an explosive motor discharge. This relief of sudden congestion by additional stimulation of other and different nerve endings is observable in the relief afforded by rubbing or stroking a part which has been pinched or bruised, or by scratching an itching spot.

<sup>1</sup> Stirling, *Ludwig's Arbeiten*, 9ter Jahrgang, p. 290; *Sitz. Ber. d. k. Säch. Gesell. d. Wiss.*, Bd. XXVI., p. 439.

<sup>2</sup> Lauder Brunton, 'On Inhibition,' 'West Riding Asylum Reports,' 1874, p. 179, and *Nature*, 1883, Vol. XXVII.

An additional causal factor in the production of tickling may lie in the nature and structure of the nervous process involved in perception in general. According to certain histological researches of recent years<sup>1</sup> we know that between the sense organs and the central nervous system there exist closely connected chains of conductors or neurons, along which an impression received by a single sensory cell on the periphery is propagated avalanche-like through an increasing number of neurons until the brain is reached. If on the periphery a single cell is excited, the avalanche-like process continues until finally hundreds or thousands of nerve cells in the cortex are aroused to considerable activity. Golgi, Ramon y Cajal, Koelliker, Held, Retzius and others have demonstrated the histological basis of this law for vision, hearing and smell, and we may safely assume from the phenomena of tickling that the sense of touch is not lacking in a similar arrangement. The importance of this law, it may be incidentally remarked, is manifest at a glance, for a future science of education. The spread of all methods whereby first-hand information is gained, while empirically found to be eminently satisfactory, is now known to rest upon a scientific basis. The laboratory method, kindergarten and primary object lessons, and constructive work, the use of illustrations in textbooks, magazines and newspapers, the stereopticon, etc., etc., may be cited as empirical recognition of this scientific fact.

May not a suggestion be offered with some plausibility, that even in ideal or representative tickling, where tickling results, say, from some one pointing a finger at the ticklish places, this avalanche-like process may be incited from central centers, thus producing, although in a modified degree, the pleasant phenomena in question? It would be in such a case another form of circular reaction.

Among the parts not mentioned by Sully as subject to ticklishness might also be mentioned the palate and the lips or any part rendered more or less sensitive, as in the case of sores. The palate, in many cases at least, may be tickled by having the tip of the tongue pass lightly backwards or forwards over its surface. In certain physical moods such ticklishness with me is almost unbearable. The reactions observable upon the recovery by a limb of its normal condition after having been 'asleep' are identical in some respects with certain phenomena of tickling. A German child remarked in my hearing that champagne 'schmeckte ebenso wie eingeschlafene Füße.'

<sup>1</sup> Ramon y Cajal, 'Einige Hypothesen über den anatomischen Mechanismus der Ideenbildung, der Association und der Aufmerksamkeit,' *Archiv für Anatomie und Entwicklungsgeschichte*, Jahrgang 1895, pp. 367 ff.

In visual and auditory perception there may be induced some of the phenomena of tickling. A medical friend of mind informs me that certain notes in deep solemn music affect his epigastric muscles in a sort of shock reaction. The quivering can be induced by false notes at times. As to the deepest causal factor, I should say that tickling is the result of vaso-motor shock. In addition to these cases the phenomena of tickling may be autogenous in nature, that is to say, vaso-motor changes may be induced in the skin without apparent external stimulation. These changes are known at times to produce the phenomena of tickling.

If hypotheses are in order, I might suggest that as the attitude of disgust and dislike may be an incipient act of vomiting or the rejection of unpalatable food, so the smile may betoken an attitude of the whole organism in which the inception of food is the most striking characteristic. These actions which are obviously so useful in matters of food may have become in the course of social evolution associated with other affairs because of their eminently social symbolic value. The lower animals must perforce express themselves somewhat differently because, according to the testimony of comparative anatomists<sup>1</sup> they lack the necessary facial muscles for language and the smile.

The laugh may have another physiological *raison d'être* besides that mentioned by Darwin, Spencer and Sully, of relief of cerebral distension and congestion. Like singing it may be a therapeutic agency in reference to pulmonary exercise, blood-oxygenation and general bodily nutrition. The deep inspirations which the singer and laughter are compelled to make cause a distension of a number of air-vessels ordinarily in a condition of semi- or almost complete collapse. As a result of the laugh the circulation is hurried on through them and the lungs are developed to their fullest capacity. The well-developed lungs, by facilitating the process of oxygenation, favor the nutrition of the body in general. The laugh, it is true, causes 'a cessation of cerebral strain,' but the greatest relief is of pulmonary or vaso-motor origin. The sigh also possesses the same function, but the difference between the sigh and the laugh is the difference between work and play. As a general rule the play activities are more general and involve a greater amount of metabolism. The vitality of play is more intense. As singing has been recommended as a valuable adjunct in the treatment of anæmia and pithinoid chests, so laughter must not be denied its therapeutic and metabolic virtues. Deep inspirations favor

<sup>1</sup> Cited by A. H. Keane, 'Ethnology.'

the flow of blood through the lungs, from the right to the left side of the heart. Thus occasional sighs or laughs, or in other words deep inspirations, interrupt the shallow breathing constituting so-called 'breathless attention.' The shallow breathing leads to stagnation of blood in the right heart, and an occasional deep inspiration is necessary to relieve this. By holding the breath for a moment the stagnation of the blood in the right heart will provoke epigastric pulsation and cause the veins in the head and neck to swell. In cases of death from suffocation or drowning, the right heart is found engorged with blood. Now in most instances of witticism or in joking, although not in all, there is an element of expectation, suspense or inhibited function. The laugh is the rehabilitation of function, the rebound to increased metabolism. This may also explain the easily-excited laugh of those attendants at a funeral or solemn ceremony where the grief is not too intense. Any foolish stimulus may cause the metabolic rebound. A friend of mine once attended an execution. The morning sun was excluded, the shadow and damp of prison walls were everywhere, the usual crowd of curiosity-mongers was present. Upon regaining the open air and sunlight the major part of the crowd burst out laughing with no other external stimulus than the exuberant sunlight.

The mechanics of laughter would also have to take into account the important influence exercised by the diaphragm, the muscular walls of the stomach and glandular activity in the various degrees of the laugh.

Sully mentions the scratching of the head during a state of mental irritation as a well-known instance of the transference of expressive movements from one state of feeling to another, à la Darwin and Wundt. Lauder Brunton explains this habit of the English rustic and similar ones, such as pulling the mustache or beard, or the German habit of slapping the side of the nose with the finger, as a stimulation of some branches of the fifth nerve, thereby causing local dilatation of the cerebral vessels and an added ability to carry on a line of thought. In a similar manner the gustatory branches and the buccal branches of the fifth nerve are stimulated by taking something that has a strong taste, such as brandied cherries. In rural regions peppermint candy is the open sesame of wakefulness in this line. Sucking and chewing and sipping are stimulants greatly increasing the flow of blood through the carotids, as has been determined by experiment. Certain elements of the smoking habit have their *raison d'être* in activity of this sort. The habit many boys have of spitting on their hands and then of

rubbing them together before taking a leap is based on the fact that thereby they obtain a sensori-muscular stimulation. Many mental and bodily automatisms usually explained by reference to some general principles such as inhibition ought to be reëxamined with the view of ascertaining the special causation in question.

Some theories die hard. Of no topic in psychology is this more true than in that of the psychology of the comic. In Sully's 'Essay on Laughter', and in the article on this subject by Hall and the present writer, may be found *in extenso* a collection of such metaphysical hard-ridden and hard-pressed definitions. Nor are Sully and other modern writers altogether free from blame in this respect. Miss Calkins in her 'Introduction to Psychology' says that 'virtually all theories of the comic agree in defining the sense of humor as enjoyment of an unessential incongruity' (p. 284). Sully says, "The most promising way of bringing the several laughable qualities and aspects of things under one descriptive head would seem to be to say that they all illustrate a presentation of something in the nature of a defect, a failure to satisfy some standard requirement, as that of law or custom, provided that it is small enough to be viewed as a harmless play-thing" (p. 139).

It is a Ptolemaic pastime trying to discover the causes and inner essence of laughter in the objective world, or even for that matter in the world of mental presentations. In the treatment of the emotions no scientific grounds for causal explanation or classification can be found in the objects of the emotions; no more can such be found for laughter, one of the prominent forms of emotion. The real causal ground of laughter is to be found in physiologic processes. A person may laugh when tickled, may laugh from the influence of drugs, may laugh automatically without the presence of mental presentations, may laugh as an exhibition of *bien être*, may laugh at a button on his coat, may laugh when there is only one single presentation in the mental field or when there are two or more. Moreover, these external things are not laughable in themselves. It is our reaction which clothes them with the cloak of humor, gayety, or what-not. In this the comic follows the general law of all emotions, including also under that term the field of æsthetics. These emotional judgments are revelations and judgments of our own selves and characters, rather than of the mountains, sculpture, paintings, or so-called laughable things. Thus the cockney's account of his exploit at a fire (quoted from the *London Times*) would hardly be yours or mine: "'Jump, yer silly fool!' we shouted, 'we've got a sheet!' and he did jump, and



there weren't no bloomin' sheet, and he broke 'is bloomin' neck. Larf! I thought I sh'd 'ave died o' larfin'." That which is high tragedy to the gods in the gallery may be comedy to the parquet, and *vice versâ*. "Avast thou wretch!" cries the demi-mondaine actress, "I would rather wear the filthy rags of poverty than don the imperial robes of sin." The artistic part of your nature laughs while your moral nature is full of pity; meanwhile there is joy in the 'nigger heaven' over another sinner repulsed.

Evidently the causal element lies in vaso-motor and nervous processes. The sense of joy present in the feeling of *bien être*, in the witicism, in the mild atmosphere of humor, is evidently due to vaso-motor phenomena and a discharge of surplus-stored energy where the discharge does not involve too much strain, effort or lesion. The laughter as a motor phenomenon may continue automatically, finally producing lesion and pain and in some cases death. In the more highly evolved form of this process, such as in wit, the element of suddenness is paramount, brought about by the coalescing of nervous currents seldom or never associated and by sudden vaso-motor and metabolic changes. In other words, we are dealing ultimately with mild forms of vaso-motor shock. Thus Dr. Edward E. Hale was taken when a boy to hear his father speak on a critical occasion. He was so impressed by hearing the orator cry: "Will any man dare say \* \* \*" that he shouted from the gallery, "No, pa!" Neither of these elements taken by themselves are laughter-producing, neither can the ideas by themselves produce such a result, but the vaso-motor shock and sudden coalescence of nervous currents may excite by association the motor centers to intense activity. The same may be said of other such instances, as for example the following: A clerk in a book store upon being asked if he had William Watson's latest poem, 'The Eloping Angels.' "The Eloping Angels," he said, scratching his head; "No, I don't think so, but we have the sequel to it, 'The Heavenly Twins.'"

In other words, it is not an appeal to our sense of superiority, to our feeling for the ludicrous, to this feeling or to that; the enjoyment we call humor or wit is the result of vaso-motor and nervous changes. The objects of the humor or wit may be numberless, or rather co-extensive with one's experience, but the fundamental or underlying process will be the same. The concept incongruity may therefore be interpreted with more propriety as the unusual. These unusual coalitions of wit and laughter, however, may at times be eminently fitting or congruous.



The laughter induced by nitrogen monoxide or by cannabis indica is probably hyperæmic or congestive in its origin. The *Rausch* in all its forms, æsthetical, political, religious, spirituous, etc., ought also to be treated in this connection. Some psychologist with Atwater courage and with no reputation to lose ought to make a study of the possible individual and social utility of the *Rausch*. The savages, it is well known, induced this intoxication by various means.

Walter E. Roth, in describing certain songs of the northern Australian aborigines,<sup>1</sup> relates an interesting fact concerning the genesis of savage emotion. He says that, "while the songs are in progress, one, two or more men — any that like — will take into their mouths, chew and spit out again, the leaves of the 'stinging tree' (*Lapartea* sp.). What with the pain and irritation so produced, such an individual is speedily aroused into a state bordering on frenzy, when he will commence eating the human excreta prepared for the purpose, will both act and give expression to anything foul and bestial he can think of, do his best to insult everybody present, start chasing the women, and, rushing hither and thither, will finally fall to the ground completely exhausted and collapsed. The mental and physical pain to which the person is thus subjected may be gauged from the fact that it requires some few weeks before he is sufficiently recovered to resume his ordinary routine of daily duties.

A most pernicious doctrine rather prevalent in theories of æsthetics and play is that of self-illusion. One author even goes so far as to say, 'Make-believe, pretence, representation, are of the essence of play, mirth, and art.' It is a case where theory and half-baked analysis run blindly against the facts. The pretence or self-illusion is in the majority of cases quite as illusive as the grin of the Cheshire cat. Sully says 'play is free activity entered upon for its own sake' (p. 146). "Play ceases to be pure play just as soon as the end, for example conquest, begins to be regarded as a thing of consequence to the player" (p. 147). Karl Groos also makes much of this theory, saying, for instance, that 'joy in conquest' is the end of play combats ('Play of Animals,' pp. 291-2).

I do not deny that there are some play-activities into which there enter pretence and make-believe, but it may be remarked at the same time that such plays are very poor play. In studying the phenomena of play two standpoints must be strictly observed, namely, the subjective and the objective. Subjectively the player, if he plays in earn-

<sup>1</sup> Walter E. Roth, B.A., M.R.C.S., etc., in Bulletin No. 4, 'North Queensland Ethnography, Games, Sports and Amusements,' Brisbane, March, 1902, p. 22.

est, that is to say if it is the best type of play, resembles closely in his activity the so-called serious occupations of life. If it were not so it would not be a useful training for after-life. Play is in many ways modeled after social life and is *the* social life for the child. It is desperately real to him, and he wonders often why adults are living such a miserable, artificial life, making money and spending wearisome laborious days for ends which are hardly worth the while. The boy who 'monkeys' or 'fools' at practice games is warned off the field by the coach. No pretence or make-believe is wanted. They play and play to win something outside the play-impulse itself. My psychology students tell me that when they play on the football field all their psychological knowledge about play being a preparation for life drops away from them and they play to win their way to the goal line. It is a serious but withal a joyous occupation to them. Such psychological knowledge may injure to some extent the complete engrossment in these preparatory occupations. The true player drops the word preparatory and simply believes these activities to be serious occupations in which he is tremendously interested. The same is true of plays of a more youthful age. The doll, for the time, is a student in school, is sick, naughty, etc. The tin horse with fore legs longer than the hind legs has longer legs; these crass adults who talk differently are talking of another world of horses. The myths and legends of the child's world are very real worlds to him.

Objectively, of course, we look upon the activities of these early stages of growth as preparatory. To call it a world of pretence is to apply a misnomer and to judge poorly of the value of play. Adults are subject to selection, so also are these preliminary stages, but it ought to be called a propædeutical selection, one by the way not yet recognized by biologists, psychologists or sociologists.<sup>1</sup>

Miss McCracken,<sup>2</sup> in speaking of the poverty-stricken girls of the working classes of a certain city, says very aptly:

"In the first place they have gone to the theatre, and they go to the theatre to see the play; not the players, nor to see how they play the play, nor why they possibly play it thus, nor why they do not play it in some other way ('in any conceivable other way,' as I overheard

<sup>1</sup> A further discussion of this topic will appear shortly under the title of 'Propædeutical Selection.' See also the writer's article on 'Play' in the *University of Colorado Studies*, Vol. 1, No. 1, and Mr. H. A. Carr's paper on 'The Survival Values of Play' in the *Investigations of the Department of Psychology and Education of the University of Colorado*, Vol. I., No. 2.

<sup>2</sup> Elizabeth McCracken, 'The Play and the Gallery,' *Atlantic Monthly*, April, 1902.

a critic murmur at a recent Shakespearean revival), nor what the author of the play meant, nor what he did not mean, nor what he should have meant. They may see all these things; they frequently do see several of them; but they go to the theatre to see the play. It is interesting to remember that in Shakespeare's time the entire audience went to see the play."

Moreover, the only true criterion of play is the performance of an activity with ease and mastery and with the spirit of pleasure. All else is work or indifferently work or play. If this thesis is granted,<sup>1</sup> then play must not be confined to what we may call traditional forms of play, but must be extended even to adult occupations when performed with the spirit of pleasure and with ease and mastery. For these reasons laughter may be classed as a form of play.

One more point only in this discussion. H. M. Stanley<sup>2</sup> and Sully<sup>3</sup> suggest that teasing may well be taken as the starting point in the evolution of play. Taking merely traditional forms of play into account, this *a priori* statement seems to be hardly warranted by anthropological data. The hypothesis seems to underlie this statement that play is a single impulse, a faculty-of-the-mind affair, whereas it is simply protean in its concrete forms. But leaving this point aside, we can safely lay claim to some actual historical data. Buecher, in his 'Arbeit und Rythmus,'<sup>4</sup> clearly proves that many songs, dances, and early forms of literature had their origin in the work activities of early men. It is needless here to repeat the evidence adduced to prove the assertion. Then, again, many ancestral adult activities have been modified to suit childish needs; many present-day adult activities are modified in the same way. But farther back than all this we may go and say that play entered in those species in which parental care began to shield their plastic young from the incidence of natural selection. Then propædeutical selection entered, whereby the preliminary, introductory, educative activities and occupations suitable to the particular species in question survived building and moulding for the larger life of the adult. That joy accompanied such a process we can reasonably believe, taking as an analogy the exuberance and fullness of life of youth wherever we find it.

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<sup>1</sup> See articles quoted above.

<sup>2</sup> H. M. Stanley, discussion of paper by Hall and Allin on 'Tickling, Laughter, and the Comic, etc.,' *PSYCHOLOGICAL REVIEW*, 1899, p. 87.

<sup>3</sup> Sully, p. 184.

<sup>4</sup> Followed, and to some extent extended by Gummere, 'The Beginnings of Poetry.'

## PSYCHOLOGICAL LITERATURE.

*Vom Fühlen, Wollen und Denken. Eine psychologische Skizze.*

THEODOR LIPPS. Leipzig, Johann Ambrosius Barth. 1902.

This sketch treats primarily of the question of feeling, and of thinking and willing only as they are necessarily involved in the discussion of the former. After defining his general position in the introduction, the writer enters upon a comprehensive analysis of feeling, indicating incidentally his views on the apperceptive processes, and somewhat more fully on the relation of feeling to the processes of volition, with a short discussion of the related questions of value and right.

In general, the feelings as subjective are opposed to 'the objective and the qualities of objective experience.' "It is the same whether I say 'I have a sensation' or 'I have a sensation of something objective,' or whether I say 'I feel' or 'I feel myself.' The whole content of sensation constitutes the perceptual picture of the objective world, just as the feelings constitute the 'I' as it is in every moment of my life immediately experienced" (p. 2).

The view of Jodl, Titchener and others, who maintain that there are only two kinds of feeling, pleasant and unpleasant, is combated; and feelings are defined as the immediate conscious indications of the relation in which individual psychic processes or events stand to the whole continuum of mental life (*psychischer Lebenszusammenhang*) (p. 5). The manifoldness of the feelings is just as great as the number of these relations, which is practically countless. The main part of the discussion is a development of this position.

Three fundamental opposites between feelings are first sketched: (1) Feelings of simple apprehension and apperceptive feelings;<sup>1</sup> the former indicates the relation in which an individual event stands to the whole apprehended content of consciousness, the latter the relation in which an event stands to the group of processes which constitutes the apperceived content of consciousness. (2) Presentative and apperceptive feelings of objectivity, and feelings of apprehension and apperception; the distinction in this second pair is based on the fact that all presentations and apperceptions are of objects, *i. e.*, on the one hand

<sup>1</sup> 'Perceptive und apperceptive Gefühlen,' p. 8.

they are presented objects, on the other they are subjective experiences. This gives rise to the double question: How do objects on the one hand, and their apprehension and apperception on the other, stand in relation to me? (3) Further, the content of my experience is determined on the one hand through my own instrumentality, on the other by objects. This is the basis of the third pair of opposites, viz., the feeling of freedom and the feeling of being determined.

The feeling of objectivity is again divided into subjective objectivity and objective objectivity, the former referring to the feeling which characterizes the simple apprehension of an object by which I find myself creating that object, the latter to those cases in which I find myself conditioned by an object; *i. e.*, there is as it were a claim put forth by the object in its own right to be perceived and I feel myself conscious of that claim. This latter, which is called 'gegenständliche Objectivitätsbewusstsein' (p. 11), is the feeling of reality.

There is a distinction made in the apperceptive processes, analogous to the distinction of freedom and determination in the way in which objects are apprehended. Corresponding to freedom of apprehension is active apperception, in which I turn my attention voluntarily to the object. Opposed to this is passive apperception, in which the object draws my attention to itself.

Returning again to the fundamental feeling, that is, to the general 'I-feeling' (Ich-gefühl), a distinction is drawn between the feeling which characterizes the state in which the attention of the subject is fixedly directed to a certain object, and that which belongs to the state in which the attention of the subject is passing from one object to another, *i. e.*, between what might be called relatively the states of rest and motion. The feeling of motion or change may be called 'Streben, Begehren, Verlangen, Erwarten, Sehnen, sich Besinnen, Wollen, Fürchten, Hoffen' (p. 19). Lipps adopts Streben (conation) to represent in general what any one of these terms might signify. This conation always involves striving towards some end, and meeting the resistance which is opposed to that process. Since in every psychic event there is a tendency towards an end, and resistance to be overcome in its progress, so every psychic event has more or less the character of conation. "And if the capacity to strive be called 'will,' using that word in its widest significance, this means nothing more than the possibility that a mental event may occur, and that resistance will be encountered in the normal progress of the event" (p. 23).

There are two pairs of fundamental opposites in the feelings of conation, based on the general distinctions drawn above: First the



feelings of subjective and objective conation; second, the feelings or active and passive conation. Active conation is my striving towards some end; aside from this is a striving 'in me,' not properly 'my striving,' *i. e.*, passive conation. This activity feeling is based on the *interest* which is at the time dominant in me.

By interest, in a general sense, is understood: "All that helps a psychic event to be apperceived — or all the factors of the psychic 'energy' of a process, *i. e.*, there must be something in me which gives an object the capability of affecting me" (p. 31). In a more particular sense, I can speak of my interests, those which direct active conation, and the interests not in this special sense mine, which support passive conation. My interests are my personality, which is the effective factor in controlling the direction of my apperceptive activity. All feelings of pleasure and value depend ultimately on the coöperation of my interests in the apperception of an object. The present ruling positive interest is in a special sense the personality, within the wider total personality. What happens aside from that which issues under the direction of this special personality is only passively mine.

The basis of the feeling of reality has already been indicated, and its development may be passed with the remark that 'the tendency in the object to appear real is at the same time my tendency to have the object appear real to me' (p. 81). Another group of feelings depends on associative preparation, *i. e.*, association has built up an expectation of what will occur under given circumstances. If a strange intrusion occur it excites the feeling of surprise, its unexpected greatness the feeling of astonishment, its suddenness the feeling of fright, etc.

The striving after knowledge, just like all other conation, arises from the accession of a subjective interest to an objective tendency. This is conditioned objectively by logical contradiction, and subjectively by my attribution of a positive value (positives Wertinteresse) to the solution of the contradiction, *i. e.*, the persistence of the contradiction contradicts, its solution corresponds to the nature of my mind. Volition is in a general sense active conation.

"The feelings of pleasantness and unpleasantness are a coloring which all feelings may take on" (p. 141). Pleasure is an immediate conscious symptom that a psychic event finds favorable conditions for its apperception, in the nature of the mind. These feelings are dependent upon two conditions: (1) The readiness in the nature of the mind to satisfy the demands which a process makes on the apperceptive activity; (2) that this readiness have an adequate opportunity to exercise itself. Whatever satisfies these conditions has the char-



acteristic of pleasantness, what transgresses them that of unpleasantness.

Approaching the ethical feelings, self-condemnation and pride are explained by reference to the nature of the structure of the personality. The 'I' is made up of an indefinite number of 'I's,' representing the passing moments of my experience. They are relatively self-dependent, but can be condensed into a single personality like so many layers of a whole. When the I of the present moment does something opposed to the expression of the total personality, there arises the feeling of self-condemnation. The development of the total personality follows the same principle as the development of the knowledge of the laws of nature, *i. e.*, modification in accord with the discovery of new facts. The feeling of oughtness (Sollen) is conation, with the character of objectivity, *i. e.*, a striving which has an objective basis, on the one hand in the claim of the object, and on the other in the positive attribution of value by the total personality. The ideal total personality is reached when all the possible experiences of the self and others, which might make revision necessary, have been added to the individual personality. The individual total-personality stands in relation to this ideal complete-personality, as the 'I' of the present stands to the total 'I.' "I have the highest feeling, *i. e.*, the feeling of unity of the ideal and the present personality, in short, the feeling of moral freedom, when my present personality is directed to the same as that which the ideal personality demands of me" (p. 196).

The name of Professor Lipps to a work on the psychology of feeling is sufficient to justify one in large expectations, and the book sketched above, though in some respects it can scarcely be considered final, amply rewards a careful study. A specially valuable characteristic of the treatment is that, without wandering afield to discuss a legion of theories, it comes directly to the point, and develops its theme logically throughout, bringing the whole into one perspective, so that the different phases appear in a definite relation to one another. Specially valuable is also his treatment of the relation of feeling to will. To say that the terminology is precisely defined, and then used consistently throughout, is only what one would expect from the author, but it is worthy of mention on account of its being a virtue so often wanting.

Professor Lipps has the failing, however, of introducing new expressions to indicate situations for which familiar terminology is quite adequate, and its use would have saved the necessary explanations, and occasionally would have left a clearer apprehension of what is ex-

pressed. To cite a single instance: what more is contained in the expression, 'gegenständliche Objectivitätsbewusstsein' (p. 11) than we understand by sense perception, and by the German *Wahrnehmung*? His chief failing is perhaps the use of terms which belong to presentational processes, to indicate different kinds of feelings, *e. g.*, feelings of apprehension, and of apperception. What more do they explain as to the actual nature of the feelings, than would be expressed by speaking of a green feeling or a white feeling? His varieties of feelings are in a number of cases indefinite just on this account, and would bear a closer analysis. And if, as is stated, 'the feelings of pleasantness and unpleasantness are a coloring which all feelings may take on' (*loc. cit.*), one might ask whether a further analysis would not indicate that the theory combated in the introduction had received a rather too summary treatment. One regrets too that Professor Lipps, in the development of his theory, makes no reference whatever to the value of the work which has been done in the investigation of the physical expression of states of feeling.

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*Histoire et solution des problèmes métaphysiques.* CHARLES RENOUVIER. Paris, Alcan. 1901. Pp. 477.

The problems of metaphysics and their solution in the French 'neocritical' philosophy were treated in their logical, systematic relations, with only such illustrative historical material as seemed necessary, in the author's previous volume, 'Les dilemmes de la métaphysique pure' (see *Psy. Rev.*, IX., 80). In the present volume this historical material is elaborated by a review and criticism of the chief systems of metaphysics as they have successively arisen in what, for want of a better term, we are accustomed to call the history of philosophy. The work, accordingly, is of the nature of a critical history of philosophy; it begins with the conception of the world in remote antiquity and comes down by the way of the great speculative constructions of ancient and modern times to the neocriticism in which the efforts to solve the metaphysical problems are held to reach, at length, a rational and satisfactory conclusion. The author, however, is very far from considering that philosophy moved towards this conclusion, as to a predetermined end, by any inherent necessity. His view of the history of philosophy is the very opposite of Hegel's. The history of philosophy is for him not the self-development of a system of thought, but the empirical succession, with, to be sure, more or less external continuity and logical connections, of warring systems, sys-

tems which, as one of his disciples (L. Dauriac) has said, can be classified, like living forms, into different and antagonistic varieties. In treating of the history of philosophy from this point of view Renouvier's chief, one might say his only, concern is with the validity of the conceptions considered. His criticism, therefore, is not what is called 'objective' and historical, as though the historical process furnished its own self-criticism, but logical and pragmatic. He is interested, above all things, in the exposition and defense of his system of thought. Hence his strictures are mainly directed to the exhibition of the inherent vice, as he conceives it, in most other systems, namely, the realization of abstractions combined with insufficiency and contradiction in the grasp and application of fundamental logical principles, defects from which his own philosophy, he claims, is free. This dogmatic interest makes him an unsafe guide on the historical side of his work; his criticisms too often lack the support of textual citations, and the student, in whom an extensive and thorough knowledge of the material is already presupposed, will find much to disagree with in Renouvier's representations. Yet, as Renouvier is unquestionably a man of genius, his remarks are always suggestive and at times profoundly penetrating; they stimulate reflection on the subject in hand even when, perhaps oftenest when, they awaken disagreement or doubt with regard to their historical accuracy. Take, for example, the following concerning Hegel: Both in Hegel and in the Neoplatonists, says Renouvier, speculation ends in the sacrifice of the individual; so that in this respect, Hegel would be reckoned in the Neoplatonic camp 'but for the fact that for him the universal is an abstraction doubly dead, wherein it is the destiny of the individual to be absorbed *without transformation into the divine*' (p. 147). Or this on Fichte: 'In the doctrine of Fichte, whose *subjective idealism* appears at first sight to have nothing to do with nature, we have to recognize in effect a sort of history of the universality of phenomena referred to their principle, which is called, but is not, the ego,' it being, as Renouvier goes on to explain, 'the noumenon of a universal ego, as if this logical essence could exist outside of positive consciousness, a person, God or man' (p. 356). Or, again, this on Kant: 'The whole Kantian doctrine of the *pure reason* tends to views concerning the deity and nature opposed to theism' (p. 338).

Probably the most valuable part of the book to a student of the history of philosophy is that in which the author takes stock of the present condition of philosophy in France (Livre X.). Among other

things, as, *e. g.*, an incisive characterization of the intellectual and sentimental temper exemplified by Renan (p. 425), it contains Renouvier's most complete account of the genesis and historical affiliations of neocriticism, particularly in its relation to Hume and Kant, to Leibniz and Descartes. This and the corresponding chapter in the *Dilemmes* contain precious material for the future historian of the philosophy of our time. And it would be quite in the order of the day, when pragmatism and pluralism are in the air and scientific monism and German transcendentalism are alike being put on the defensive by insistent claims of individuality and personal will, for some bright candidate for the doctorate in one of our universities to turn aside for once from the elucidation of the everlasting Kant or the resuscitation of some obscure and well-forgotten ancient and give attention to these Renouvierian books, with a view to a critical account and estimate of this aggressive system of phenomenalism conjoined with monadism, of critical rationalism united with rational belief. Whoever attempts this task will find a multitude of problems on his hands, not the least being one of great interest to the theoretical psychologist, namely, the determination of the conception of the soul as the law of phenomena having for its function perpetuity in time and such an original liberty of action as implies real contingency in the objective order of events (pp. 451, 460 ff.).

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#### ETHICS.

*La Morale : Fondement psycho-sociologique d'une conduite rationnelle.* G. L. DUPRAT. Bibliothèque Internationale de Psychologie Expérimentale. Paris, Octave Doin. Pp. 385.

M. Duprat conceives of ethics, not as a science, but as an art, though an art founded on a scientific study of facts. Having satisfied himself at the outset of his inquiry, that duty essentially demands the most perfect possible coördination of all functions, alike within individuals and among the individuals composing society, he concludes that the chief task of the ethical investigator is to study psychic and sociological activities, for the purpose of detecting defects of coördination, and constructing the individual and social ideals of perfect coördination. "If we discover [constatons] incompatible tendencies, vices, defects and excesses that mar the harmony of the whole and the coördination of individual or collective functions, it is our duty [as ethical investigators] to indicate what should be suppressed, what should be

developed, what created, in order that the system may become at once the richest and the most harmonious possible" (p. 57).

With regard to this program two chief doubts suggest themselves. It may well be asked whether the welfare, the real and genuine interest, of the conscious living beings making up society is not of more fundamental moral importance than anything so purely abstract as coördination. Order, coördination, organization, is indeed indispensable for the promotion of the welfare of beings living in close contact with one another, but the intrinsic value of mere arrangement seems to be æsthetic rather than ethical.

And, again, when ethics undertakes to diagnose the ills of the social body, to prescribe remedies, and to set up the ideal of social health, as Dr. Duprat says it should, the particularity of this undertaking may well render it precarious. To be sure, M. Duprat prepares himself for his difficult task by careful studies of the conditions of individual and social action, as set forth by psychology and sociology, and of the trend of social evolution, as set forth by the latter science, upon which he leans heavily. But it still remains true that personal conviction is largely responsible for Professor Duprat's construction of the individual and social ideals offered as the *summum bonum*.

In short, the author, in seeking to work out an account of morality on the theory that ethics is an art, commits himself to undue dependence on knack, skill, insight, and other subconscious, untestable procedures for the attainment of his results. At least so it seems to the present writer, and so, he believes, it will seem to the careful reader of Dr. Duprat's book.

But it should also be pointed out that the book discusses in an interesting way family, industrial, political and educational ideals, considering, among other live problems, ideal family relations, relations of labor and capital, scope of governmental functions, and coeducation.

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*Der æsthetische Genuss.* KARL GROOS, Professor der Philosophie an der Universität Giessen. J. Rickersche Verlagsbuchhandlung, Giessen, 1902.

This new work appears in the place of a second edition of the author's earlier 'Einleitung in die Aesthetik.' It differs, however, in so many important respects from what the author calls his *Jugendversuch*, as to amount practically to a new piece of work. In the first

place, the author confines himself to a study of the more general conditions of æsthetic enjoyment and appreciation, reserving for a later work his chapters on the 'modifications of the æsthetic,' and for a still later treatise his study of the processes of æsthetic creation. But in addition to this difference in scope there is a noticeable difference in method. The psychological standpoint is more rigidly observed, and there is a decided redistribution of emphasis, a toning down of those aspects of his earlier work which were distinctively his own and a tendency to take account critically of all the recent contributions to the subject, thus making the later work less individual and more representative.

The most noticeable redistribution of emphasis appears in connection with the rôle given to the concepts of play, inner imitation and illusion. The difference may perhaps be best expressed by the statement that, while these functional attitudes are still conceived to be fundamental as distinguishing the æsthetic from the non-æsthetic, the pleasure values connected with these activities are not given the important weight which they received in the earlier work. On the other hand, much more emphasis is put upon the content side, upon the pleasure values or effectiveness of the direct and reproductive factors, sensational and ideal. The proportionally large number of pages given to the detailed treatment of the sensational and reproductive factors in æsthetic effect, their fusions and colligations, is significant of this change of method.

In his treatment of the direct or sensational factor a large place is given to the muscular and organic sensations, leading even to the claim that 'the specifically æsthetic disposition presupposes a strong motor type, and that with the reduction of the motor elements, there is reduction of æsthetic effect.' In treating of the sensational factor he also makes an important distinction between the *pleasant* and the *intensive* effects, making a basis for such distinctions as that between the beautiful and the sublime. The intensive effects are largely the product of the fusion of intense organic sensations with other content. Although on this point his views are not so definite as one might wish, it is here that the emotional values of the work of art are to be found, it would seem. The chapter on the reproductive factors in æsthetic effect describes the most important conditions of the *Verwachsung* (this is the author's own term) of sensational and associative factors, and the most important forms of this interpenetration of the two elements. The sphere of associated content is widened to include feelings and emotions, but here again the lack of clear definition of the emotional element is felt.



The play impulse, inner imitation, or voluntary surrender to the play of impressions, and the state of illusion, are still, as we have indicated, fundamental criteria of the æsthetic, but the æsthetic value is conceived to lie not so much in a distinct pleasure value arising from these activities, as in the intensification through them of the pleasure values arising from the greater fusion of the sensational and reproductive factors. The activity of play does indeed have its pleasure value (p. 112) as illusion has its pleasure value, independent of the values of the sensational and reproductive factors; but 'the chief value of illusion (p. 228) lies in the fact that it deepens, indeed makes possible even, the reactive feelings which belong to the content.' It is in connection with the concept of illusion and *Scheingefühle* that Groos has been most influenced by criticism, notably that of Külpe and Lipps, and has so modified his conceptions as to avoid the serious psychological difficulties to which they gave rise. Moreover, in connection with the study of inner imitation he has introduced the concept of *Einfühlung*, and his analysis of these phenomena is one of the most important individual features of the work.

Another modification of importance is his introduction of a chapter upon the æsthetic judgment. Distinguishing between the æsthetically effective and æsthetically valuable, or between æsthetic effect and worth, he conceives the latter to be a narrower province within the former. While the normative as such lies outside psychology, the discussion of the motives determining æsthetic value judgments is a psychological problem. Moral and knowledge judgments intermingle with æsthetic and contribute to the total effect. Within the æsthetic judgment itself are motives and postulates which go beyond the merely sensational and reproductive factors, with their pleasure values. The typical, the perfect, the purposeful suggestiveness of human meaning, all enter in to distinguish the æsthetically valuable from the merely effective. Groos does not go beyond the mere psychological description of these motives; he nevertheless furnishes a critical sundering of these judgments which, in turn, through fusion with the sensational and associative elements become factors in the total æsthetic effect.

In general, it may be said that this last attempt of Professor Groos to deal with general æsthetic principles represents a distinct advance in the direction of a general æsthetic theory. His own more specialized work during the years which have intervened since the publication of the '*Einleitung*,' together with the careful consideration given to the results of other workers in the field, have made possible a work

which, if less individual than the earlier publication, is certainly nearer the ideal of a general scientific treatise. The modifications and redistribution of emphasis which the various elements of his theory have undergone have in no way affected their importance and value, but are only such as a broader view of all the phenomena of the field in question made desirable. In its present form, Groos' view of the entire field of the psychological conditions of the æsthetic appreciation is perhaps as nearly authoritative as any can be in the present state of the subject.

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*Les Obsessions et les Impulsions.* A. PITRES et E. RÉGIS. Paris, Octave Doin. 1902. Pp. 434. (Bibliothèque Internationale de Psychologie Expérimentale.)

The authors of the 'Report on Obsessions' before the International Medical Congress in Moscow, in 1897, develop their theses in the present volume into a systematic treatise. So complete a treatment of the subject has, we believe, not before appeared.<sup>1</sup> The work is characterized not so much by the development of novel views as by general soundness of view resting on an extraordinarily large experience. The authors tell us that within some ten years they have personally observed no fewer than four hundred cases of obsessions. A large number of these are here reported. The book certainly conveys a very vivid impression of the facts.

The view taken of obsession is that it is a morbid state fundamentally emotional. It is defined as a morbid syndrome characterized by involuntary and distressing consciousness of parasitic sentiments or thoughts which tend to impose themselves on the ego, developing within the subject, in spite of his efforts to expel them, creating thus a sort of psychical dissociation whose final term is the conscious doubling, or division, of the personality (p. 16). This conception serves to distinguish obsession from the fixed idea. The fixed idea in its pure form is an intellectual phenomenon, gives rise to no distress and revolt, is accepted, for the most part, as real. Pure obsession, on the other hand, is a condition of emotional distress not derivable from any antecedent or accompanying idea, and when an idea is present it is recognized as false or at least pathological (p. 17). The two phe-

<sup>1</sup> Since the above was written, P. Janet's *Les Obsessions et la Psychasthénie*, a still more elaborate work, has been published. Critical notice will appear in a later number of the REVIEW. Janet, while recognizing the relative importance of the emotional theory of obsessions advocated by our authors, criticises it as in certain respects vague and incomplete.

nomena are often, however, associated, and it is admitted that in practice it is not always easy to say which gives rise to which. Obsessions, in the broad sense, are accordingly classified into phobias and obsessions (in the narrower sense), phobias being again subdivided into diffuse and special and obsessions into ideative, impulsive and hallucinatory. This classification of phobias along with obsessions, which departs from the common representation, is based on the view that the two are but varieties or degrees of the same neuro-psychopathic condition and that they differ only in the proportion in which the emotional and ideational elements are combined and in the way in which they severally develop. Obsession, it is maintained, is often only an aggravated or intellectualized form of phobia (p. 66). It is unfortunate that the carrying out of this view, which seems to have much to commend it, should introduce a somewhat perplexing confusion of terminology.

In addition to the description and illustration of the most important forms of phobia and obsession, the authors select for detailed discussion the obsession of blushing, a form of obsession which they first described in 1896 and which they claim had never been seriously and systematically described before. The obsession of blushing (ereuthophobia) is distinguished from the mere morbid and distressing tendency to blush (emotional ereuthosis) by the presence of a fixed idea (p. 180). It is said to be commoner in men than in women. The fact that, once the obsession created, the crises are usually brought on by the idea, might be held to conflict with the view that the fundamental characteristic of obsession is the emotional disturbance. But this is not necessary. What the subjects speak of as the 'idea' of blushing is, properly speaking, a fear. It consists, according to the authors, in a vivid representation, a systematic hypermnesia of affective memory and a spontaneous revival of a previously painful emotion, leading, unless checked, to the reproduction of the original emotion (p. 182). This view is confirmed by the reflex-like suddenness with which the crises come on in certain cases and by the fact that, under favorable conditions of the atmosphere, it is sometimes found impossible to blush, no matter how much the idea of blushing may be entertained. The experiments, therefore, which show the order (1) phobic idea, (2) blushing, (3) distress, accord in general with clinical observation, but they do not prove that the phenomenon is primarily intellectual; if (2) and (3) came first, (1) would doubtless follow. The primary disposition would seem to be regarded as a certain affection of the vaso-motor system; for the authors believe that the tendency to

blush habitually precedes the emotional distress and that frequently differences in the intensity of the phobia bear a relation to the disposition to blush. To this extent they accept Lange's view of the importance of the vaso-motor element in the emotional process while refusing to commit themselves to his general theory (p. 195).

In the matter of etiology it is maintained that the habitual predisposing cause is heredity. Usually the obsession develops suddenly on occasion of some accidental event and is most frequently determined by some form of moral emotion. The view that it is exclusively of sexual origin is rejected. The disputed question as to whether it may ever terminate in insanity is decided in the affirmative. The final conclusion as to its nature is that it is not a distinct disease, but a symptom, and this is the general opinion. In the words of the authors, it "can be regarded as \* \* \* one of the elements constituting a special emotional syndrome, the syndrome of acute distress (*angoisse*) liable to present itself in different degrees and under various forms in different neuroses and psychoses" (p. 252). Two chapters dealing respectively with the diagnosis and treatment of obsessions bring this portion of the book to a close.

The last third of the book treats of morbid impulses. In introducing the subject, the authors remark on the prevailing confusion respecting it, some regarding the 'impulsion' as an irresistible or at least imperious morbid tendency, some as a forced act, some both indiscriminately; some, again, consider it as exclusively a conscious obsessive propensity, while others admit that it is sometimes unconscious and automatic. The view of it taken here is that it is simply the tendency to reflex action breaking down the normal voluntary *tonus*, *i. e.*, the harmonious regulation of the instincts by the ego. Hence the term is here used to cover all pathological impulses from the blind, automatic impulse at the one extreme to the obsessive impulse in its most attenuated form at the other. The execution of the act is not regarded as indispensable nor the impulse itself as necessarily irresistible. It is defined as 'the imperious and often irresistible tendency to revert to the pure reflex' (p. 291). Morselli is followed in the description of the general characteristics of morbid impulses as endogenous, imperious, aberrant, involuntary and usually, though not always, conscious. The one selected as the basis of classification is that of constraint. Thus three types are distinguished: (1) pure motor, wholly reflex; (2) psycho-motor, quasi-reflex; (3) psychic, or impulsive obsessions. Morselli is again followed in the classification of clinical types, the chief of which, impulses to suicide, homicide, theft,

arson, drink, wandering away and sexual acts, are described seriatim at some length. These types are regarded not as monomanias, but as symptoms or syndromes of a variety of psycho-pathic conditions — degeneration, epilepsy, hysteria, etc. — in each of which they take on a modified form. Among the points of interest in the discussion of the medical aspects of the phenomena, the view taken of the treatment by suggestion may be referred to as indicating the present trend of opinion on this subject. The authors believe that this form of treatment is only applicable in cases of impulsive obsessions and even then only when there is a hysteropathic disposition for hypnotic suggestion to work on; but even in these cases they find the results frequently disappointing in spite of the large promise of the beginning. But the pure degenerate and the victim of constitutional neurasthenia — and these form a large part of the cases — cannot, they declare, be hypnotized at all.

The work concludes with the detailed report of a number of criminal cases in which the authors have given expert testimony in favor of medical treatment in place of the ordinary legal penalties.

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*Human Nature and the Social Order.* CHARLES HORTON COOLEY.

New York, Charles Scribner's Sons. 1902. Pp. viii + 413.

It is the author's aim 'to set forth from various points of view, what the individual is, considered as a member of a social whole.' Believing that "'society' and 'individuals' do not denote separable phenomena, but are simply collective and distributive aspects of the same thing," he holds that in current thought, the social and the individual are often opposed in a fallacious fashion, as though two separable entities or forces were under consideration.

Our modern world is particularly liable to emphasize the individual aspect as in some sense more real, or as psychologically primary though ethically inferior. Such abstract views may be classed as: (a) *mere individualism*, in which the collective phases are looked upon as quite secondary and incidental; (b) *double causation*, in which society and the individual are thought of as separate causes, and socialism is opposed to individualism as a theory of life; (c) *primitive individualism*, in which man is conceived to have been originally a mere individual, but to have been undergoing a process of socialization, and thereby of moralization, since the individual in this view is liable to be identified with the bad, the social with the good; (d) the



*social faculty view*, in which the social is regarded as including only a part of the individual, *i. e.*, certain faculties or emotions. As contrasted with these four views the author holds that individuality and sociality have always existed as complementary aspects, and that 'the line of progress is from a lower to a higher type of both, not from the one to the other'; that 'man's psychical outfit is not divisible into the social and the non-social, but that he is all social in a large sense, is all a part of the common human life, and that his social or moral progress consists less in the aggrandizement of particular faculties or instincts and the suppression of others than in the discipline of all with reference to a progressive organization of life, which we know in thought as conscience.'

It is evident from the above that the author's program includes both social psychology and ethics. The platform just quoted is of course not peculiar to the author. It has been gradually emerging in the work of criticism and construction in which many have engaged. The interesting and valuable feature of Professor Cooley's contribution is the happy manner in which he has developed and illustrated his thesis. He has had the fortune to have two children so different in their types of reactions as to prevent the hasty generalizations from 'one' to 'all' sometimes met with in genetic psychology, and has added important observations and reflections to this field. He has drawn upon a wide acquaintance with literature for illustrations of the social sentiments, and has indicated ways in which the experimental method might well be applied to this great field of emotional expression. He has an observant eye for significant aspects of human conduct, and sanity of judgment for their interpretation. He has expressed himself with simplicity and directness. The various points of view from which he considers his subject, as suggested by the titles of his chapters, are suggestion and choice, sociability and personal ideas, sympathy, the social self, hostility, emulation, leadership, the social aspect of conscience, personal degeneracy, and freedom.

Under 'Suggestion and Choice' the phenomena of imitation are discussed. The point is well taken that we must distinguish carefully between the result and the process. The result of a child's efforts may be the production of an act which is like the act of the parent, but the process may not be at all that of mechanical suggestion. Children 'cannot imitate an act except by learning how to do it, any more than grown-up people can, and for a child to learn a word may be as complicated a process as for an older person to learn a difficult piece on the piano.' The two children observed by Professor Cooley



showed a marked difference as to imitation. One showed scarcely any tendency of imitation until after the age of two years and a half ; the other began to show vocal imitation when a little over two months and continued in the ordinary path. The former reached his results largely by his own experimentation and refused to imitate directly.

The chapter on ' Sociability and Personal Ideas ' makes a vigorous statement of the fact that society is fundamentally a subjective reality. " In order to have society it is evidently necessary that persons should get together somewhere ; and they get together only as personal ideas in the mind." " Persons and society must, then, be studied primarily in the imagination." " The imaginations which people have of each other are the *solid facts* of society." It seems to the reviewer that the emphasis upon the function of imagination, both in this and the following chapter, is one of the best features of the book. The chapters on the social self may be regarded as in part a supplementation of Professor James' chapter on the self.

The author has a special criticism in several passages upon the popular antithesis between egoism and altruism. It may be readily granted that the phrases as ordinarily employed are liable to criticism in so far as they seem to imply mutually exclusive alternatives. But common life knows perfectly well what it means by selfishness, and no amount of psychological criticism on the terminology will alter the fact that one man has a ' self ' which does not recognize the equal or superior claims of others, while another man has a self which does recognize the claims of others to be treated always ' as an end, never as merely a means.' Now, from the individual standpoint one of these selves may be as ' harmonious,' as ' stable ' as the other. But they have very different ethical values. I query whether, in his desire to correct a current misuse of terms, the author has done justice to the real ethical difference implied in the terms. Other queries as to details will naturally suggest themselves to the reader, but the book is a decidedly welcome contribution to social psychology.

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#### NATURE OF CONSCIOUSNESS.

*The Psychology of Mental Arrangement.* I. MADISON BENTLEY.  
Am. Journ. Psychol., Vol. XIII., No. 2.

" We may say, in general, that any structure made up of heterogeneous elements — if it be anything more than a mere collection or heap — implies arrangement, plan, pattern, and not simply addition of

abstracts units." The question is whether this arrangement or principle of synthesis is to be found without or within the sum of the units. The problem of mental arrangement is, in the last analysis, the problem of interpreting our psychological abstractions in terms of the steps by which these abstractions were first made and have since been elaborated. In the associational and faculty psychology this, only too obviously, never was done. Is it done in the recent psychology which discusses the problem?

This paper aims 'to bring together the various recent contributions to the subject \* \* \* by the Austrian school of psychologists' (Ehrenfels, Meinong, Cornelius, Witasek, Schumann and others). Ehrenfels insists that over and above the elements into which a mental complex can be analyzed there is a factor of arrangement which he calls the form-quality (*Gestaltqualität*). Meinong uses the term funded-contents (*fundierte Inhalte*). Cornelius, on the other hand, starts with 'a big, unanalyzed, undissected mass' (*Gesamteindruck*). Schumann, Stumpf and Stout represent intermediate positions.

The real problem here involved is the fundamental one: When is experience one, and when is it many? Under what conditions do we view consciousness as a unity, and when do we distinguish various aspects or elements? It is essentially the same as the question when mind is to be viewed statically as a thing or entity and when dynamically as a process, a stream, or movement. These are questions concerning consciousness which can be answered only by a functional analysis from within, never by a survey from without. It must be an internal rather than an external type of analysis. The whole discussion shows "how misleading is such a rubrication as 'sense' and 'intellect.' Mind is not so simple as that one can say of a mental formation, 'this is either a sensation that has found its way into mind or a sheer spiritual creation that mind has evolved.'" It is true 'that a complete descriptive account of a mental complex demands more than an enumeration of its constituent elements,' but on the other hand, in these discussions, "where a 'funded' factor has been found necessary, the true nature of psychological elements has not, as a rule, been kept in view," and the concept of 'mental activity,' here as elsewhere, is ambiguous.

One phase of the discussion is the insistence by certain writers on the existence of distinct elements of relation, a consciousness of 'togetherness.' "Is the 'fourness' in the perception of four objects, or the 'squareness' of the square itself, an element?" Is 'liquidity'

something more than pressure + temperature? Does the 'fifthness' of the fifth remain the same though the elements be changed? It appears so. Stout says that 'togetherness' or, as he calls it, 'the apprehension of form,' is a distinct kind of consciousness, a 'constituent of consciousness comparable \* \* \* with the perception of red or blue.'

The solution of this problem is to be found in an examination and criticism of our methods of psychological analysis and abstraction. If our method yields us an abstract particular, psychological atoms, then, to offset this, we are compelled to postulate some synthetic principle, some abstract identity. This is the fallacy underlying the uncritical use of such terms as 'mental activity,' 'attention,' and the various phrases by which this Austrian school of psychologists express the important idea of mental arrangement as over against the mental units which result from the analysis of any concrete experience.

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*The Unity of Process in Consciousness.* HENRY RUTGERS MARSHALL. *Mind*, New Series, Volume XI., pp. 470-502.

Looking at the world of living organisms, from the standpoint of an outside observer, one 'cannot but be impressed with the evidence that a unity of process exists through all the apparent diversity which he at first observes in the forms of living bodies.' Thus it appears, at least, to the biological investigator whom we may conceive to view this process as "a 'spirit,' if we may use the term, with such full capacity for scientific observation and analysis as man displays, but without any thought that his observations or analyses are aught else than interesting modifications of his consciousness; without any knowledge that his consciousness is related to any human body; and with no notion whatever that any form of consciousness has any connection with animal activities."

'Our spirit' observes that if any bit of matter, whether lifeless or alive, receives a stimulus, it reacts upon this stimulus and becomes a different thing. In the case of living matter, there is, apparently, the capacity to become again what it was before its reaction to the stimulus. But a closer inspection shows that this capacity is never exactly regained. Reaction to a stimulus never leaves the body exactly what it was before the reaction. In the case of the complex organism and environment this is called 'learning by experience.' Probably it is characteristic of all living matter, but escapes observation in the sim-

pler forms because of the inadequacy of our observation. That is, there 'must be a unity of process in all the reactions of living matter.'

Close observation, furthermore, fails to reveal any absolute distinction between the multicellular organism and the single cell. The cell is a complex, a system, a living machine, as truly as what we call the organism; and the activity of any part involves changes in the whole mass. "The activity of the system as a whole is in fact merely differentiated in form by this special activity of the part." Here, again, is evidence of a unity of process in the nature of life as exemplified in animal behavior, a unity which embraces the differences rather than excludes them.

As a test case, let us suppose that 'our spirit' turns his attention to the examination of the reactions of the nervous system of civilized man, which reveals animal behavior in, perhaps, its most complex form. Here, too, a close study shows 'that no reaction in any part of a system of systems can fail to modify in some measure the total pulse of activity in the whole system of systems.' "Stimulation and reaction occur in one and the same act." Stimulus, response, and the intermediate central nervous process are simply phases of an organic circuit. They are not successive stages but coördinate constituent functions in one process. The 'neurergic pattern' which the whole system or circuit presents depends simply upon what aspect is emphatic at the time.

'Our spirit,' however, may turn his attention to apparent variations from this. "He notes for instance in some cases immediacy of reaction to a stimulus, and in others a marked hesitancy." "Under certain conditions of stimulation he sees what appear as trial of one form of activity with failure, then of another with success, and a persistence of the successful form of activity." But here again 'he sees that the basis of the adaptation of the activity of a complex system of systems to new conditions must always lie in the emphasis of some partial activity in some minor system of the great system of systems; and that the capacity to effect this emphasis of a partial activity is one which inheres in all systems, whether relatively simple or complex, and that when effected it must give rise to a variation from the reaction which he has looked upon as typical.'

So far, this unity of process has been found and described by 'our spirit' in purely objective terms, such as the scientific biologist might use. "We may now assume that he suddenly, and for the first time, discovers the startling fact that his own mental states are in some way related to a human body." He first asks of what he is really thinking

when he says that his mind has a body. Take a concrete case. "I have a certain spatial presentative experience of an 'object in the outer world' which I call a sharp-pointed pin; and this is presented as approaching what is another 'object in the outer world' held in the same presentation, and which latter I call the finger of this human body. When in this complex presentation the pin touches this finger I experience not only the complex spatial presentation of the two objects in contact, but also what I may speak of as a streak upon the surface of the stream of consciousness, and which I call a painful pricking sensation; and this is a non-spatial presentation, it is not an 'object in the outer world.'" "I observe so many striking facts of this kind, and the evidences of the relation thus suggested are so many, that I am led to assume for the moment an hypothesis which I shall call 'parallelism.'" "If I think at all of the relation between the action of nerve in my body and my non-spatial presentation I cannot, under this hypothesis, assume the occurrence of a non-spatial modification of presentation without also assuming the existence of a coincident 'action of nerve' in my body." "I perceive at once that logically I should expect to be able to assert the reverse of this proposition, that is, I ought also to contend that I cannot assume any action of nerve in my body without assuming also a coincident modification of my non-spatial presentative experience."

There will be forced upon the attention of our introspective psychologist (for such 'our spirit' has now become), the fact of the unity of process in nerve action with which he has already become familiar as a scientific biologist. The continuity in the stream of consciousness is reflected in or is reflection of the unity of process in nerve action. And the neural systems within systems he finds reflected in the fact that consciousness in like manner is a complex system of psychic systems. Moreover, he finds that there is a 'noetic pattern' corresponding with the 'neurergic pattern,' and that he must view this 'noetic pattern,' also, "as a whole pulse of psychic activity, and these especially vivid presentations merely as partial psychic activities which for the moment are especially emphatic." "And thus he sees how what may really be a unity of psychic process, in a complex system of systems, may appear at the first glance to be a diversity of process."

But, is the converse true? Is all nerve action accompanied by a coincident psychic modification? Similar arguments lead to an affirmative answer. 'There is some modification of psychic life in connection with all action of nerve,' and this is 'mentality.' "There



must always be coincident a system of mentalities which under certain conditions becomes what we call a consciousness."

This, it seems to the reviewer, is the central problem. What are these 'conditions'? And why call these systems *mental* unless or until these conditions have been fulfilled? The real logic of Mr. Marshall's article seems to be this: In organic function we find and describe unity and continuity of life process, expressed in the idea of growth. The unity and continuity of function in the life process imply orderly and continuous activity with reference to an end. But the idea of end has meaning only in or with reference to conscious experience. Therefore, in consciousness alone is to be found real unity of process. Whether this is idealism or not is an entirely distinct question, depending upon one's conception of the nature of consciousness.<sup>1</sup>

The assumption of unconscious mental states seems only confusing. We have already the convenient category of the neural or the physiological for the unconscious conditions of consciousness. Why then create a realm of the unconscious psychical to account for them? Why would it not be as legitimate to postulate a subphysical world to account for difficulties in physics as to posit a subpsychical realm to account for these unconscious conditions of consciousness?

Two further points: It seems to the reviewer that Mr. Marshall distinctly weakens his argument by his defense of the 'back-stroke' theory of the emotions. His own skilful analysis of the organic circuit seems rather to imply (what he denies) that 'the data for consciousness are in all cases supplied through afferent channels.' But very timely seems the criticism of Mr. Morgan's introduction of 'effective consciousness,' to account for the higher forms of animal behavior. "The principal objections to the view that consciousness at times 'enters in' to guide behavior, and at times does not, lies in its unacknowledged denial of the unity of process in consciousness."

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#### EXPERIMENTAL.

*Eye-movements and the Aesthetics of Visual Form.* G. M. STRATTON. *Philosophische Studien*, Bd. XX. (Wundt's *Festschrift*, II. Thiel), pp. 336-359.

The substitution of photographic records of eye-movements for introspective testimony as to the way in which the eyes behave is

<sup>1</sup> See a forthcoming article on 'The Functional Theory of Parallelism' by the present writer in the *Philosophical Review*, where this thought is developed.



without doubt a long step in advance. Photographic methods as applied by Professor Dodge to the measurement of reaction times, and similar methods as applied by Professor Stratton in this investigation to the determination of the direction of eye-movements, show how utterly unreliable are all the statements based on mere introspection. Professor Stratton finds that in following the outlines of simple figures, such as circles and rectangles, and various kinds of curves, the eye does not make a movement corresponding to the line followed, but moves forward in a series of irregular and often wholly ungraceful lines. A circle, for example, is followed by movements that describe a triangle or an irregular closed figure. A rectangle is followed in both curved and unequal straight movements. Graceful curves and ungraceful irregular lines are followed by the eye in movements hardly distinguishable from each other. These facts lead Professor Stratton to the conclusion that eye-movements do not contribute the chief factors to our recognition of æsthetical forms.

Æsthetical form is due rather to a wider process of synthesis. The regular curve is more gratifying to us than an irregular line because its regularity economizes our attention. We can apprehend its purpose or principle as a fixed and unitary formula of direction. The wavering and uncertain purpose of the ungraceful line is by contrast uneconomical of attention. Among the various regular lines, curves have more of the suggestion of life. The graceful flight of birds and the movements of the skater, or similar forms of living movement, come vaguely into consciousness when we feel the beauty of a graceful curve. There may be an organic vasomotor or muscular response as a secondary sensuous basis for this association with living movement.

Æsthetical form is thus seen to be not merely a matter of sensation. It is a matter of general interpretation and involves all phases of mental nature. Indeed, our author finds in his study opportunity to emphasize the relation of the æsthetical to the moral and religious purposes of life.

The theory of æsthetical form here outlined has broad implications which we shall expect to find Professor Stratton working out in greater detail in the future. The present article is exceedingly suggestive in its conclusions; in its earlier experimental part it is a large contribution to our knowledge of eye-movements.

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*Die Arbeitscurve.* EMIL KRAEPELIN. Philos. Studien, XIX. (Wundt's Festschrift, I.), pp. 458-507.

It is difficult to summarize this article, which is itself a summary of a considerable portion of the research that has been done in Professor Kraepelin's laboratory for something like ten years.

The work that has been especially employed for study is that of adding columns of figures, the successive combining of two numbers serving as a test of the amount of work done. An extended period of work shows marked variations in the rapidity of execution from beginning to end, which are especially striking when plotted into a curve. The work aims to explain these variations. This is accomplished by analyzing the curve into component parts, each of which represents a fundamental process. These processes vary with the individual, so that the methods for analyzing the curve serve also to analyze the subject's mental constitution.

The two most pronounced factors in shaping the curve are fatigue (*Ermüdung*) and practice (*Uebung*). Fatigue tends to decrease the rate of working, and hence to make the curve drop from the beginning of the period. With subjects easily fatigued this is often the result, but subjects not so sensitive to this factor show a gradual rise for a considerable time, when finally all alike show a rapid fall caused by an increased difficulty in work as a result of a high degree of fatigue. The gradual rise is due to practice, which tends to make a performance more quickly and easily executed the more it is repeated. This influence overcomes the immediate effects of fatigue, and accumulates during a single working period, increasing also in a marked way the rate of working for each successive period. Although loss from lack of practice is more rapid at first than later, traces from it have been found to remain for months; so that with frequent repetitions the rate of adding will constantly increase until a stage is finally reached at which improvement stops. By comparing several subjects it has been found that great ease of fatigue, rapidity of improvement by practice and a quick loss from lack of practice are characteristics which go together.

Since fatigue is recovered from more quickly than practice is lost, there is a point reached after the stopping of work at which one is able to work with the greatest rapidity. The rest up to this point is called 'the most favorable pause' on account of its effect upon work which immediately follows. The increase of working power is not constant, however, from the beginning to the end of this pause, because of a momentum (*Anregung*) from the work, which lasts some ten minutes.

This arises from a sort of mental inertia, which prevents the rapidity of work from reaching its maximum at first, as well as continuing the working efficiency after the work has stopped. The duration of this momentum is found by varying the pause between two periods of work. The pause which is followed by the slowest rate marks the point at which the momentum is completely lost.

Another factor which influences the curve, especially at first, is adaptation (*Gewöhnung*) to the condition of the experiment and the various disturbances which figure in the surroundings. This tends to delay the rapidity of working, but after two or three days ceases to be a disturbing factor, as more complete mental concentration is attained. Yet another element of the curve of work arises from the effort (*Antrieb*) incident to starting the work of adding. This voluntary effort is difficult to maintain and shortly disappears. When most apparent it causes a rapid fall in the curve from the point of starting. If more than a couple of minutes at the beginning of the period are averaged for the first section of the curve, the effect is likely to be covered up because of its brevity. Effort is also often consciously present when a disturbance threatens the mental concentration, and also near the end of a period when anticipated release causes the subject to accelerate his speed.

A lithographic cut presents in distinct though somewhat imaginative proportions the curve elements which compose the curve of work.

These results are a good illustration of what persistence and well directed effort will accomplish in the way of solving a very knotty problem. Although this is not fully solved, there are the best of reasons for anticipating its complete solution in the near future. As a means to this end may be suggested more perfect experimental conditions to meet the delicacy of the problem. Complete control of auditory and visual impressions which in practically all laboratories make complete 'adaptation' impossible, could be accomplished by means of suitable apparatus in a dark room removed from auditory shocks. Adding, too, though the most satisfactory means for measuring work yet employed, has disadvantages. Five and one, *e. g.*, are more quickly added than seven and nine. Either carefully arranged columns, in which easy and difficult additions are systematically mixed, or the substitution of some other work, as perhaps counting, would greatly add to the uniformity of results.

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## NEW BOOKS.

- Human Nature and the Social Order.* C. H. COOLEY. New York, Scribners. 1902. Pp. viii + 413.
- The Psychology of Ethics.* D. IRONS. Edinburgh and London, Blackwoods. 1903. Pp. xviii + 176.
- Heredity and Social Progress.* S. N. PATTEN. New York and London, Macmillans. 1903. Pp. 215.
- Nietzsche et l'Immoralisme.* A. FOUILLÉE. 2<sup>me</sup> éd. Paris, Alcan. 1902. Pp. xi + 292. 5 fr.
- L'Homme préhistorique.* S. ZABOROWSKI. 7<sup>me</sup> ed. Bibioth. utile. Paris, Alcan. No date. Pp. 187. 60 c.
- Essai sur l'Hyperespace, le temps, la matière, et l'énergie.* M. BOUCHER. Paris, Alcan. 1903. Pp. 204. 2 fr. 50.
- The Light of China.* I. W. HEYSINGER. Philadelphia, Research Pub. Co. No date. Pp. 165.
- A metrical rendering of the *Táo Teh King* of Láo Tsze (604-504 B. C.).
- What is Meaning?* V. WELBY. London and New York, Macmillans. 1903. Pp. xxxi + 321.
- Grundzüge der physiologischen Psychologie.* 5th ed., in three volumes. Vols. I., II. Leipzig, Engelmann. 1902. Pp. xv + 353, viii + 686. M. 10 and M. 13.
- Notice is reserved until the appearance of Vol. III.
- L'Hypnotisme et la Suggestion.* Dr. GRASSET. Bibl. Int. de Psych. Expér. Paris, Doin. 1903. Pp. 534. 4 fr.
- Von der Natur der Dinge an sich.* W. K. CLIFFORD. Trans. by H. KLEINPETER. Leipzig, Barth. 1903. Pp. 48. M. 1.20.
- Leib und Seele. Der Entwicklungsgedanke.* Zwei Reden, 2. Aufl. C. STUMPF. Leipzig, Barth. 1903. Pp. 72.
- Die Lehre vom Denken.* A. BASTIAN. I. Th. Berlin, Dümmler. 1903. Pp. 211. M. 5.
- Vorträge in Besprechungen über die Krisis des Darwinismus, etc.* By members of the 'Philos. Gesell. an der Univ. zu Wien.' Leipzig, Barth. 1902. Pp. 77.

*Die Schrift bei Geisteskrankheiten.* Ein Atlas mit 81 Handschriftproben. R. KÜSTER; Vorwort von R. SOMMER. Leipzig, Barth. 1903. Pp. vii + 169. M. 10.

*Proceedings of the American Association for the Advancement of Science* (Pittsburg, 1902). Pub. by the Sec. 1902. Pp. 620.

*Geist und Körper, Seele und Leib.* L. BUSSE. Leipzig, Durr'sche Buchh. 1903. Pp. x + 484. M. 8.50.

An extended defense of the 'Interaction' theory and criticism of other views.

*Publications of the Bureau of Ethnology, Smithsonian Institution. Tsimshian Texts.* F. BOAS. Washington, Gov. Printing Office. 1902. Pp. 244.

*Modern Spiritualism.* F. PODMORE. 2 vols. London, Methuen; New York, Scribners. 1902. Pp. x + 307, xii + 374. \$5.

*Human Personality and its Survival of Bodily Death.* F. W. H. MYERS. London and New York, Longmans, 1903. Pp. xlv + 700, xx + 660. \$12.

*Vom Fühlen, Wollen und Denken.* TH. LIPPS. Leipzig, Barth. 1902. Pp. viii + 196. M. 6.40.

*Variation in Animals and Plants.* H. M. VERNON. New York, Holt. 1903. Pp. ix + 415.

*Pure Sociology. A Treatise on the Origin and Spontaneous Development of Society.* LESTER F. WARD. New York, Macmillans, 1903. Pp. xii + 606.

*Spinoza's Political and Ethical Philosophy.* ROBERT A. DUFF. Glasgow, James Maclehose and Sons. 1903. Pp. xii + 516.

*Le mensonge.* G. L. DUPRAT. (Biblioth. de Philos. Contemp.) Paris, Alcan. 1903. Pp. 190.

*Gesammelte Abhandlungen zur Physiologischen Optik.* ARTHUR KÖNIG. Leipzig, J. A. Barth. 1903. Pp. viii + 443.

*Quarto-centennial Celebration, University of Colorado, November 13, 14 and 15, 1902, Boulder, Colo.* (Univ. of Colorado Bulletin, Vol. II., No. 4, Dec., 1902). Pp. 112.

*L'année biologique, Sixième année, 1901.* YVES DELAGE. Paris, Schleicher. 1903. Pp. lxxxiv + 575.

*More Letters of Charles Darwin.* Edited by FRANCIS DARWIN. New York, Appletons. 1903. 2 vols. Pp. xxiv + 494, 508. \$5 per vol.



*Essai de classification naturelle des caractères.* CH. RIBÉRY.  
Paris, Alcan. 1902. Pp. xxiv + 199.

*The Study of Mental Science.* J. BROUGH. London, Longmans,  
Green. 1903. Pp. 129.

*Experimental Studies in Psychology and Pedagogy.* Edited by  
LIGHTNER WITMER. *I. Spelling in the Elementary School: An  
Experimental and Statistical Investigation.* OLIVER P. CORN-  
MAN. Pp. 98. *II. The Sensation of Pain and the Theory of  
the Specific Sense Energies.* ANNA J. McKEARG. Pp. 87.  
Boston, Ginn & Co. 1902.

*Life and Letters of the Rt. Hon. Friedrich Max Müller.* Edited  
by his WIFE. Two vols. London and New York, Longmans.  
1902. Pp. xiii + 534, ix + 521.

The preface starts out: "It may be thought that the publication of these volumes is superfluous after the two works, *Auld Lang Syne* and the *Autobiography*, written by Max Müller himself. But it seemed that something more was wanting to show the innermost character of the real man. \* \* \* The object of this book is to show 'the elevation of soul and enlargement of mental outlook which was revealed more and more as his life's work opened before him.'" Truly a pious task and no doubt worth while — if we are to have three lives of Stevenson! The book is redolent of the Oxford setting, and that is 'to the good' for those who find Max Müller an overrated individual who never lost the personal and dramatic cue in his life's rôle. Why should 'the Rt. Hon.' be put in the title of a great man's biography? Think of Darwin or Spencer needing such an introduction! Intrinsically the volumes are of very great interest, and as specimens of book-making they rank high. J. M. B.

*Carnegie Institution of Washington. Yearbook No. 1, 1902.*  
Washington, The Carnegie Institution. 1903. Pp. xlv + 305.

The report of the Advisory Committee on Psychology, included in this yearbook, is reprinted in the *Princeton Contributions to Psychology*, Vol. IV., No. 1, now in press (Princeton, N. J.). We doubt the wisdom of holding such a report for sale (\$1); we understand that even press copies are not sent out. We have received an advertising circular. These reports would be most instructive to the public, and the general diffusion of such information would seem to fall within the scope of the Foundation. We hope the trustees will not adopt a too commercial policy with their publications. In the announcement of the *Index Medicus*, revived by the Carnegie Institu-



tion, it is said too of that publication, that it will not be sent, in exchange, to other journals, but sold — at a rate that seems likely to restrict its circulation greatly.

J. M. B.

*Outlines of Cosmic Philosophy.* JOHN FISKE. Introduction by JOSIAH ROYCE. Boston & New York, Houghton, Mifflin & Co. 1903. Four vols. Pp. cxlix + 276, 411, 373, 390.

Professor Royce's 'Introduction' confines itself largely to the exposition of what Fiske actually said—to a sort of epitome of the 'Cosmic Philosophy' in its relation to the author's other books. This seems unfortunate because, as Professor Royce himself says, Fiske's opinions are now mainly of historical interest, and no one would read the 'Cosmic Philosophy' for current views. The reprint would have had greater justification if the 'Introduction' had attempted to bridge the gulf—to develop and modify Fiske in the directions of later scientific investigation. As it is, the present writer confesses that he does not see the utility of the reprint, except indeed in so far as there may be a demand for the complete writings of Fiske on the part of some who are not able to procure the old edition of this work. Professor Royce's 'Introduction' does have interest, but yet it seems somehow to lack sympathy or vitality—characters which arise, at least in part, from the straitness of its scope.

J. M. B.

*Les obsessions et la psychasthénie.* II. F. RAYMOND and PIERRE JANET. Paris, Alcan. 1903. Pp. xxiv + 543. 14 fr.

A collection of clinical lectures following upon the Vol. I. by Janet, with the same title. It renews the collaboration begun by the same authors in the second volume of *Névroses et idées fixes*.

*History of the Problems of Philosophy.* JANET and SEAILLES. Trans. by A. MONAHAN, with Introduction by H. JONES. Vol. I. *Psychology*. Vol. II. *Ethics, Metaphysics, Theodicy*. London and New York, Macmillans. 1902.

This book will undoubtedly be useful in English, especially the volume devoted to psychology. Yet we share the hesitation shown by the editor, Professor Jones, as to its meeting the full needs of English-speaking students. It has two distinct claims to recognition: it is — that is, the psychological volume is — the only historical work in English coming down beyond Kant; and it is full where the German treatises are empty. Furthermore, we have great sympathy with the author's protest against being criticised for not doing what he did not set out to do. The most serious defect of the psychological volume is, perhaps, its incompleteness — due to the arbitrary selection of topics.

J. M. B.

## NOTES.

It is with regret that we record the death of Professor D. G. Ritchie, of St. Andrews. Both chairs in philosophy at that university thus become vacant at once. The Scottish universities have had great losses recently, by death and removal, in this department—Fraser, Sorley, Stout, Knight, and earlier Caird resigned; Adamson and Ritchie removed by death.

WE are also called to note the resignation of Professor Clark Murray, of McGill University, Montreal.

WE regret to hear of the death of Professor Hiram M. Stanley, of Lake Forest University, which occurred on April 3.

DRS. C. A. STRONG and Livingston Farrand, of Columbia University, have been appointed full Professors of Psychology and Anthropology, respectively, in that institution.

MISS M. F. WASHBURN, Professor in the University of Cincinnati, has been appointed associate Professor of Philosophy at Vassar College.

THE *Revue de Philosophie* announces the publication of an annual *Index philosophique*, beginning with 1902 (to appear March, 1903). It will include 'Psychology (with the related subjects, Physiology, Medical Science, Biology),' as well as Logic, Metaphysics, Ethics, and History of Philosophy (169 rue de Rennes, Paris, VI<sup>e</sup>; 3 fr. 50, or to subscribers to the *Revue*, 2 fr. 50).

DR. F. S. WRINCH, of Princeton University, has been appointed to a Research Assistantship by the Carnegie Institution, for research to be conducted in the Princeton laboratory, under the direction of Professor Baldwin.

MESSRS. CHARLES SCRIBNER'S SONS announce that they have arranged for the publication of a 'Library of Historical Psychology,' under the editorial supervision of Professor Baldwin of Princeton University. The library is to comprise a series of volumes written by leading authorities at home and abroad on the history of the various leading topics of psychological thought from the earliest times, each volume being an independent work, but the whole constituting an encyclopedic 'History of Psychology.' The arrangements for the volumes of the library—of which there will be twelve or more—are now being perfected, and the publishers expect to make early announcement of certain of the titles, names of writers, etc.

